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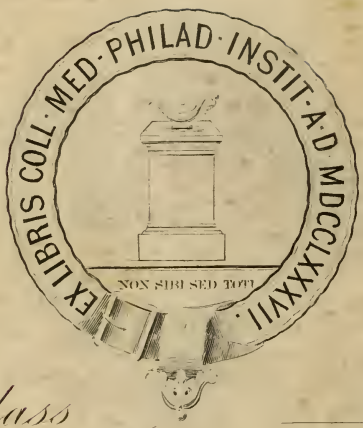

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Founder.—TAYLOR, HUGH M., M.D. President of the Virginia Board of Medical Examiners. Richmond, Va.

1892.—THOMPSON, J. E., M.D. Professor of Surgery in the University of Texas.

1891.—TIFFANY, LOUIS MCLANE, M.D. Professor of Surgery in the University of Maryland; Fellow of the American Surgical Association. 831 Park Ave., Baltimore, Md.

1890.—TOMPKINS, CHRISTOPHER, M.D. Professor of Obstetrics in the Medical College of Virginia. Richmond, Va.

1891.—TRIPPE, RICHARD JEFFERSON, M.D. President of the Chattanooga Medical Society. Chattanooga, Tenn.

1891.—VANCE, AP MORGAN, M.D. Fellow of the American Orthopedic Association. Louisville, Ky.

Founder.—VAUGHAN, BOLIVAR A., M.D., Columbus, Miss.

1891.—WALKER, EDWIN, M.D., Ph.D. Gynecologist, Evansville City Hospital; ex-President Indiana State Medical Society; Fellow of the American Association of Obstetricians and Gynecologists. Evansville, Ind.

Founder.—WATHEN, WILLIAM HUDSON, M.D. Professor of

Abdominal Surgery, Obstetrics, and Gynecology in the Kentucky School of Medicine; ex-Chairman of the Section on Obstetrics and Diseases of Women, American Medical Association; ex-President of the Kentucky State Medical Society; Consulting Gynecologist to the Louisville City Hospital; Fellow of the American Gynecological Society and of the American Association of Obstetricians and Gynecologists. Louisville, Ky.

1888.—WESTMORELAND, WILLIS F., M.D. Professor of Surgery, Atlanta Medical College. Atlanta, Ga.

Founder.—WELLFORD, JOHN SPOTSWOOD, M.D. Professor of Diseases of Women and Children, Medical College of Virginia; President of the Richmond Academy of Medicine. Richmond, Virginia.

1891.—WHEAT, LOUIS, M.D., Richmond, Va.

1890.—WILSON, HENRY PARKE C., A.M., M.D. Ex-President of the Medical and Chirurgical Faculty of Maryland and of the Baltimore Academy of Medicine, and of the Baltimore Gynecological and Obstetrical Society; Surgeon to the Hospital for Women of Maryland; Consulting Gynecologist to St. Agnes Hospital; Consulting Surgeon to the Johns Hopkins Hospital; Fellow of the British Medical Association, and of the British Gynecological Society; ex-President of the American Gynecological Society. Baltimore, Md.

Founder.—WILSON, J. T., M.D. Ex-President of the North Texas Medical Association. *Vice-President,* 1891. Sherman, Texas.

1891.—WYETH, JOHN ALLAN, M.D. Professor of Surgery, New York Polyclinic; Consulting Surgeon to St. Elizabeth Hospital: Visiting Surgeon to Mt. Sinai Hospital and Polyclinic Hospital; Honorary Member of the Texas State Medical Association; ex-President of the New York Pathological Society. 294 Madison Ave., New York City.

1889.—YANDELL, DAVID W., M.D. Professor of the Principles of Surgery and Clinical Surgery in the Medical Department of the University of Louisville; ex-President of the American Surgical Association; ex-President of the American Medical Association. Louisville, Ky.

CONSTITUTION.

ARTICLE I.

The name of this Association shall be The Southern Surgical and Gynecological Association.

ARTICLE II.

The object of this Association is to further the study and practice of surgery and gynecology among the profession of the Southern States.

ARTICLE III.

This Association shall adopt and conform to the Code of Ethics of the American Medical Association.

ARTICLE IV.

Any reputable physician, who practises surgery or gynecology, and who is vouched for by two members of the Association, and recommended by the Council, shall be eligible to membership in this body.

ARTICLE V.

Section 1. The officers of this Association shall be a President, two Vice-Presidents, a Secretary, a Treasurer, and a Council, elected by ballot, without nomination.

Sec. 2. The President and Vice-Presidents shall be elected for one year, and the President shall not be eligible for re-election at any time; the Secretary and Treasurer, each, for five years, and the Council as provided for in the By-laws.

ARTICLE VI.

SECTION 1. It shall be the duty of the President to preside at all meetings of the Association; to give the casting vote; to see that the rules of order and decorum be properly enforced in all deliberations of the Association; to sign the approved proceedings of each meeting, and to approve such orders as may be drawn upon the Treasurer for expenditures ordered by the Association.

SEC. 2. In the absence of the President the first Vice-President shall preside, and in his absence the second Vice-President shall preside.

SEC. 3. In the absence of all three, the Association shall elect one of its members to preside *pro tem.*

SEC. 4. It shall be the duty of the Secretary to keep a true and correct record of the proceedings of the meetings; to preserve all books, papers, and articles belonging to the archives of the Association; to attest all orders drawn on the Treasurer for moneys appropriated by the Association by the seal of the Association; to keep the account of the Association with its members; to keep a register of the members, with the dates of their admission and places of residence. He shall collect all moneys due from the members, and pay to the Treasurer, taking his receipt for the same. He shall report such unfinished business of previous meetings as may appear on his books requiring action, and attend to such other business as the Association may direct. He shall also supervise and conduct all the correspondence of the Association, and be chairman of the Publishing Committee.

SEC. 5. It shall be the duty of the Treasurer to keep a correct record of all moneys received from the hands of the Secretary, giving his receipt for the same; pay them out by order of the Association, as indorsed by the President and attested by the seal in the hands of the Secretary.

SEC. 6. It shall be the duty of the President of the Association to appoint a Committee of Finance, consisting of three members of the Association, whose duty it shall be to examine the books of the Secretary and Treasurer, and report on the same at the next session of the Association.

ARTICLE VII.

Vacancies occurring in the officers of the Association shall be filled by appointment of the President until the next meeting. He shall also have the appointment of all committees not otherwise provided for.

ARTICLE VIII.

This Constitution shall take effect immediately from the time of its adoption, and shall not be amended except by a written resolution, which shall lie over one year, and receive a vote of two-thirds of the members present.

ARTICLE IX.

Any member who for three consecutive years fails to attend the meetings shall be dropped from the roll of membership.

BY-LAWS.

ARTICLE I.

The Southern Surgical and Gynecological Association shall meet annually on the second Tuesday of November at 10 A.M., at such place as may be designated by the preceding meeting.

ARTICLE II.

The members present shall constitute a quorum for business.

ARTICLE III.

The annual dues of each member shall be $10, paid in advance.

ARTICLE IV.

The usual parliamentary rules governing deliberative bodies shall govern the business workings of this Association.

ARTICLE V.

All questions before the Association shall be determined by a majority of the votes present.

ARTICLE VI.

The President shall deliver an annual address at each meeting of the Association.

ARTICLE VII.

The Secretary of the Association shall receive, at each annual Session, a draft from the President, drawn on the Treasurer, for the sum of $100 for services rendered the Association, and to this shall be added the necessary expense incurred in the discharge of his official duties.

ARTICLE VIII.

It shall be the duty of the Secretary, one month prior to the annual meeting, to notify by circular letters the members of the Association, and urge their attendance.

ARTICLE IX.

The authors of all essays shall notify the Secretary, six weeks prior to the meeting, of the titles of their essays, so that they may be incorporated in the circular letter to the members.

ARTICLE X.

COUNCIL.

The Council shall consist of five members; and of those elected at the primary meeting, the first shall serve five years, the second four, the third three, the fourth two, and the fifth one year; so

that subsequently one member of the Council shall be elected annually to serve five years. The President and Secretary shall be *ex-officio* members of the Council.

This Council shall organize by electing a Chairman and Secretary, and shall keep a record of its proceedings.

The duties of this Council shall be—

1. To investigate applications for membership and report to the Association the names of such persons as are deemed worthy.

2. To take cognizance of all questions of an ethical, judicial, or personal nature, and upon these the decision of the Council shall be final; *provided*, that appeal may be taken from such decision of the Council to the Association, under a written protest, which protest shall be sustained by the Association, and the matter shall then be referred to a special committee, with power to take final action.

3. All motions and resolutions before the Association shall be referred to the Council without debate, and it shall report by recommendation at as early an hour as possible.

ARTICLE XI.

The following are the standing committees of the Association to be filled by the President and to report at the next annual meeting subsequent to their appointment, viz.: Committee of Arrangements and Committee of Publication. The Committee of Publication shall consist of three members, one of whom, the Secretary of the Association, shall also be the chairman of the said committee. The Committee of Arrangements shall consist of five members.

ARTICLE XII.

The Committee of Publication shall have full power to omit from the published *Transactions*, in part or in whole, any paper that may be referred to it by the Association, unless specially instructed to the contrary by the Association, which will be determined by vote.

ARTICLE XIII.

No honorary members shall be placed upon the roll of this Association.

ARTICLE XIV.

Any member failing to pay his dues for more than one year shall be dropped.

ARTICLE XV.

Every physician making application for membership shall present with his application a paper on some surgical or gynecological subject.

ARTICLE XVI.

No paper shall be read before this Association which does not deal strictly with a subject of surgical or gynecological importance.

ARTICLE XVII.

No paper read before the Association shall be published in any medical journal or pamphlet for circulation, as having been read before the Association, without having received the indorsement of the Committee of Publication.

ARTICLE XVIII.

The reading of papers shall be limited to twenty minutes each, except by permission of the Association.

MINUTES OF THE PROCEEDINGS

AT THE

FIFTH ANNUAL MEETING

OF

THE SOUTHERN SURGICAL AND GYNECOLOGICAL ASSOCIATION,

HELD IN THE

COUNCIL CHAMBER OF THE CITY HALL,

Louisville, Kentucky,

NOVEMBER 15, 16, AND 17, 1892.

FIFTH ANNUAL MEETING.

The following-named members were present:

BALDRIDGE, M. C.	Huntsville, Ala.
BAXTER, GEO. A.	Chattanooga, Tenn.
BLUE, W. R.	Louisville, Ky.
BRIGGS, WM. T.	Nashville, Tenn.
BROKAW, A. V. L.	St. Louis, Mo.
BROWN, BEDFORD	Alexandria, Va.
CAIN, JOHN S.	Nashville, Tenn.
CARTLEDGE, A. M.	Louisville, Ky.
CECIL, JOHN G.	Louisville, Ky.
COBB, W. H. H.	Goldsboro, N. C.
COCHRANE, HARDIN P.	Franklin, Tenn.
CUNNINGHAM, R. M.	Birmingham, Ala.
DALE, JOHN R.	Arkadelphia, Ark.
DAVIS, WM. ELIAS B.	Birmingham, Ala.
DIXON, ARCHIBALD	Henderson, Ky.
DUGAN, WM. C.	Louisville, Ky.
EVANS, JAS.	Florence, S. C.
FORD, DESAUSSURE	Augusta, Ga.
GASTON, JAS. McF.	Atlanta, Ga.
GRANT, HORACE H.	Louisville, Ky.
GRIFFITHS, GEO. W.	Louisville, Ky.
HAGGARD, W. D.	Nashville, Tenn.
HALL, RUFUS B.	Cincinnati, Ohio.
HOGAN, S. M.	Union Springs, Ala.
HOLMES, J. B. S.	Rome, Ga.
JELKS, J. T.	Hot Springs, Ark.
JOHNSON, J. T.	Washington, D. C.
JOHNSTON, GEO. B.	Richmond, Va.
KELLY, H. A.	Baltimore, Md.
KOLLOCK, CORNELIUS	Cheraw, S. C.

LETCHER, J. H.	Henderson, Ky.
LYDSTON, G. FRANK	Chicago, Ill.
McGUIRE, H. H.	Richmond, Va.
McMURTRY, L. S.	Louisville, Ky.
MARCY, H. O.	Boston, Mass.
MATHEWS, JOS. M.	Louisville, Ky.
NICOLSON, WM. P.	Atlanta, Ga.
PALMER, E. R.	Louisville, Ky.
POTTER, W. W.	Buffalo, N. Y.
REAMY, T. A.	Cincinnati, Ohio.
REED, CHAS. A. L.	Cincinnati, Ohio.
RICKETTS, EDW.	Cincinnati, Ohio.
ROBERTS, WM. O.	Louisville, Ky.
RODMAN, W. L.	Louisville, Ky.
ROSS, GEO.	Richmond, Va.
THOMPSON, J. E.	Galveston, Texas.
VANCE, AP MORGAN	Louisville, Ky.
WALKER, EDW.	Evansville, Ind.
WATHEN, WM. H.	Louisville, Ky.
WESTMORELAND, W. F.	Atlanta, Ga.
YANDELL, DAVID W.	Louisville, Ky.

Letters and messages of regret were received from a number of members who were prevented from attending the meeting.

First Day—*Tuesday, November* 15, 1892.

Morning Session.—The meeting was called to order by the President, Dr. J. McFadden Gaston, of Atlanta, Ga., at 10 a.m.

Dr. L. S. McMurtry, of Louisville, Chairman of the Committee of Arrangements, delivered the following "Address of Welcome:"

Mr. Chairman and Gentlemen of the Southern Surgical and Gynecological Association: It affords me great pleasure to welcome you to our city. This Association, now beginning its fifth annual meeting, has already demonstrated its claim to usefulness, and also has demonstrated that there was an important office for it to fill. That it has filled that office acceptably and well has been demonstrated by the constantly growing interest in its sessions, and also in the constantly increasing numbers of its

membership and in the growth of its annual volume of *Transactions.*

It is unnecessary for me, Mr. Chairman, to say a word to this Association, except to felicitate it, as a member working in its ranks, upon the happy manner in which our *Transactions* have been received by the medical press not only in this country, but in Europe. So that I cannot let this opportunity pass without congratulating the Association upon the admirable work it has accomplished, and upon the strong hold it has upon the teaching of the profession of the country at large, and particularly of the Southwest.

The large associations devoted to surgery and gynecology hold their meetings annually in the great accessible railroad centres of the Eastern and Northern States, hence to carry surgery and gynecology South, which have grown to such grand, magnificent proportions of late years, it was necessary that the Southern States should have an organization devoted exclusively to these branches.

Mr. Chairman, it is eminently proper that the fifth annual meeting of this Association should be held in this city. In the early part of this century Southwestern medicine established its Mecca at the Falls of the Ohio. Here we have had for many years, and do have now, medical schools with large attendance, and have here a great educational centre for the Southwestern States. Here in this city where we are now assembled, labored Samuel D. Gross, Austin Flint, and Henry Miller in the early days of their manhood, when they were building the foundation of those great reputations that have left their names prominent upon the history of medicine and surgery in our country, and have left their labors as a matter of felicitation to all of us who live in Kentucky. In this State, Mr. Chairman, there have been men who have grown up and labored, who have laid the foundations of some of those important branches in the new surgery which it is the office of this Association to cultivate. I allude to McDowell, to Dudley, and others.

It gives me pleasure to state that the medical profession of Louisville extend to this body of gentlemen a most cordial welcome to our midst. I assure you that the profession of this city have learned with great pleasure of your coming; and we will show you before you leave us, that it is a great honor to us to

have you meet with us and conduct your deliberations in our midst and give us an opportunity to hear you talk.

Mr. Chairman, I have the pleasure to announce that, recognizing the precedent established by this Association in regard to entertainments, your Committee of Arrangements have deemed it best not to prepare elaborate entertainments, or to make the social features, however delightful they will be to you, interfere with the scientific deliberations. We recognize the fact that you are here to make a book, to bring matters to be discussed, and that the object of your coming together is for discussions, and anything which detracts from the attendance and interest in your proceedings diminishes the size of your book, and, what is worse, shortens and impairs the extent of the labors of this Association and the work that it will do.

Entertainments.

At 1.30 P.M. to-day Dr. and Mrs. Ap Morgan Vance will be pleased to receive the members of the Association for luncheon; also visitors. This evening at 8 o'clock I request the presence of the Fellows at an informal reception at the Pendennis Club. To-morrow (Wednesday) at 1.30, Dr. and Mrs. W. O. Roberts request the company of the Fellows for luncheon at their residence on Second Street. Wednesday evening, from 8 to 11 o'clock, the profession of Louisville request your presence at a reception at the Galt House.

RESPONSE BY THE PRESIDENT.

Gentlemen of the Association: We will accept the most cordial invitations extended to us by the Chairman of the Committee of Arrangements, Dr. McMurtry, and the statements he has made will be considered in all respects official, and the invitations as cordial as though received by written notice.

Dr. McMurtry announced that a meeting of Southern Medical Colleges would be held at one of the hotels at 8 P.M., with a view to consider a preliminary programme for business, the object being the lengthening of the term of medical courses.

The President then declared the Association ready for the transaction of its scientific business.

Papers were then read as follows:

1. "Personal Recollections of the Late Dr. Benjamin W. Dudley, of Lexington, Kentucky, and His Surgical Work," by Dr. Bedford Brown, of Alexandria, Va.

Dr. Brown at the close of his paper urged that a place in the *Transactions* be allotted to place on record the works and deeds of prominent Southern men.

The Secretary agreed with Dr. Brown, and said it was very appropriate to have sketches of the deceased illustrious members of the profession published in the annual volume of *Transactions*, and he knew the Association would congratulate Dr. Brown upon the selection of Dr. Dudley for this occasion.

Owing to the large number of papers on the programme the Secretary moved that the reading of papers be limited to twenty minutes, in accordance with the By-laws, and the discussions on the same to five minutes. Seconded.

Dr. G. Frank Lydston moved, as an amendment, that each member be limited to a single discussion of the same paper.

Dr. Davis accepted the amendment. The motion was then put by the President and carried.

2. "Experiences in Pelvic Surgery," by Dr. A. V. L. Brokaw, of St. Louis, Mo.

Discussed by Drs. Potter, Johnson, Davis, and Brokaw.

President Gaston, as President of the Association and in behalf of the members present, extended an invitation to all the members of the local medical profession and visitors to attend the sessions and to participate in the discussions.

3. "Craniotomy upon the Living Fœtus is Not Justifiable," by Dr. Cornelius Kollock, of Cheraw, S. C.

Discussed by Drs. Haggard, McGuire, McMurtry, Dixon, and discussion closed by the author.

4. "A Case of Extensive Hæmatocele Resulting from Tubal Pregnancy Rupturing into the Broad Ligament," by Dr. W. D. Haggard, of Nashville, Tenn.

Discussed by Drs. Marcy, Johnson, Jelks, Lydston, Davis, and discussion closed by the essayist.

5. "Fibroid Tumor of the Uterus; Pregnancy; Rupture at Fourth Month; Operation Six Weeks Afterward; Death," by Dr. S. M. Hogan, of Union Springs, Ala.

Discussed by Drs. Jelks, Kollock, Johnson, Gaston, and Davis, and discussion closed by Dr. Hogan.

On motion, the Association adjourned till 2.30 P.M.

Afternoon Session.

The Association reassembled at 2.30 P.M., and was called to order by the President.

6. "A New Operation for the Radical Cure of Inguinal Hernia," by Dr. George A. Baxter, of Chattanooga, Tenn.

7. "Treatment of Umbilical and Ventral Hernia," by Dr. William H. Wathen, of Louisville, Ky.

8. "Surgical Treatment of Inguinal Hernia in the Male," by Dr. Henry O. Marcy, of Boston, Mass.

These three papers were discussed jointly by Drs. Gibney, of New York (by invitation), Kelly, Hanks, of New York (by invitation), Jelks, Brokaw, Ricketts, Davis, Brown, Gaston, Cartledge, Kollock, McMurtry, and discussion closed by the essayists.

Dr. Marcy exhibited specimens in connection with his paper, and entertained the Association with numerous lantern slides of herniæ.

The President appointed Drs. McMurtry and Haggard to serve on the Council in the absence of Drs. Hardon and Engelmann.

On motion, the Association adjourned till Wednesday morning.

SECOND DAY—*Wednesday, November* 16, 1892.

Morning Session.—The Association was called to order at 9.30 A.M. by the President.

9. "Treatment of Ununited Fractures," by Dr. W. O. Roberts, of Louisville, Ky.

10. "Symptoms of Fracture: Their Importance and Significance," by Dr. W. C. Dugan, of Louisville, Ky.

These two papers were discussed jointly by Drs. Brown, Gaston, Letcher, Kollock, McIntyre, Potter, Kelly, Evans, Briggs, Reamy, Lydston, Nicolson, Vance, Greenly (by invitation), Baxter, and discussion closed by the essayists.

The President appointed as an Auditing Committee, Drs. Potter and Holmes.

11. "I. Some Complications of Ovariotomy: With Cases. II. Abdominal Hysterectomy: With Specimens," by Dr. L. S. McMurtry, of Louisville, Ky.

Discussed by Drs. Marcy, Kelly, Reamy, Eastman, Reed, Hall, Lydston, Dugan, and discussion closed by the author of the paper.

The Council, through Dr. J. S. Cain, made its report on the names presented for membership, and the following were recommended by the Conncil and elected members of the Association:

Griffiths, George W. . . .	Louisville, Ky.
Hunter, H. M.	Union Springs, Ala.
Cecil, John G.	Louisville, Ky.
Barrow, David	Lexington, Ky.
Hall, Rufus B.	Cincinnati, Ohio.
Grant, Horace H. . . .	Louisville, Ky.
Palmer, E. R.	Louisville, Ky.
Thompson, J. E. . . .	Galveston, Texas.
Chapman, John T. . . .	Bessemer, Ala.
Blue, W. R.	Louisville, Ky.
Dixon, Arch.	Henderson, Ky.
Simons, F. Grange . . .	Charleston, S. C.
Briggs, C. S.	Nashville, Tenn.
Briggs, Waldo	St. Louis, Mo.
Prince, D. Malloy . . .	Laurinburgh, N. C.
Rodman, W. L. . . .	Louisville, Ky.

12. "Intestinal Anastomosis by a New Device," by Dr. H. Horace Grant, of Louisville, Ky.

Discussed by Drs. Brokaw, Davis, and Lydston,

On motion, the Association adjourned till 2.30 P.M.

Afternoon Session.

The Association reconvened at 2.30 P.M., and was called to order by the President.

The First Vice-President, Dr. Cornelius Kollock, was called to the chair, after which the President proceeded to deliver the Annual Address. He selected for his subject "Compatibility of Conservative and Aggressive Surgery."

The Address received marked attention, and the speaker here and there was warmly applauded.

At the close Dr. Baxter moved that a vote of thanks be extended to the President for his able, timely Address. Carried.

13. "A Plea for More Rapid Work in Surgery," by Dr. Ap Morgan Vance, of Louisville, Ky.

Discussed by Drs. Dixon, Hall, Baxter, and Ricketts.

14. "Surgery of the Ureter, with a Report of Cases," by Dr. Charles A. L. Reed, of Cincinnati, Ohio.

Discussed by Drs. Dixon, Kelly, Ricketts, McGuire, Eastman, Brown, and discussion closed by the essayist.

15. "Specialism as Related to the Practice of Surgery and Gynecology," by Dr. William Warren Potter, of Buffalo, N. Y.

16. "The General Practitioner as a Gynecologist," by Dr. R. M. Cunningham, of Birmingham, Ala.

17. "The Morphology of Large Ovarian and Myomatous Tumors," by Dr. Howard A. Kelly, of Baltimore, Md.

Dr. Kelly illustrated his paper by numerous lantern-slide illustrations.

On motion, the Association adjourned until Thursday morning.

Third Day—*Thursday, November* 17, 1892.

Morning Session.—The Association was called to order by the President at 9.30 A.M.

18. "The Present Status of Drainage in Surgery," by Dr. A. Morgan Cartledge, of Louisville, Ky.

Discussed by Drs. Kelly, McGuire, Marcy, Eastman, Reed, Baxter, Westmoreland, Ricketts, Myers, McIntyre, Davis, Jelks, and discussion closed by the essayist.

19. "The Surgical Treatment of Tubercular Peritonitis," by Dr. William H. Myers, of Fort Wayne, Ind.

Discussed by Drs. McIntyre, Cunningham, Kollock, Baxter, Brown, Dixon, Wathen, and closed by the author of the paper.

20. "Specialism in Medicine," by Dr. Willis F. Westmoreland, of Atlanta, Ga.

21. "Bacteriological Research in Its Relations to the Surgery of the Genito-urinary Organs," by Dr. G. Frank Lydston, of Chicago, Ill.

Discussed by Drs. McGuire, Jelks, and discussion closed by the essayist.

22. "Ovariotomy in Old Women," by Dr. Joseph Taber Johnson, of Washington, D. C.

23. "The Simple, Septic, Traumatic, and Specific Forms of Cervicitis, and their Treatment," by Dr. Bedford Brown, of Alexandria, Va.

Discussed by Drs. T. B. Greenly (by invitation), Jelks, McIntyre, and discussion closed by the author.

24. "Shock," by Dr. James Evans, of Florence, S. C.

Discussed by Drs. Cartledge, Ricketts, Kollock, Cunningham, Baxter, Roberts, and, in closing, by the essayist.

25. "A Manipulative Mistake and Its Consequences," by Dr. James Ross, of Richmond, Va.

26. "Operation for Hare-lip without the Use of a Pin," by Dr. William Perrin Nicolson, of Atlanta, Ga.

Discussed by Drs. Roberts, Cartledge, Dugan, Kollock, Briggs, and discussion closed by the essayist.

27. "Cholecystotomy, with Report of a Case," by Dr. Edwin Ricketts, of Cincinnati, Ohio.

Discussed by Drs. Eastman, Dixon, Gaston, Davis, and, in closing, by the author.

Report of the Auditing Committee.

GENTLEMEN: The committee appointed to audit the financial statements of the Secretary and Treasurer respectfully report that the accounts and vouchers have been duly examined and found correct in every particular and detail.

Respectfully submitted,

WILLIAM W. POTTER,
J. B. S. HOLMES,
Auditing Committee.

Dr. McGuire offered the following resolution:

Resolved, That it is the sense of the Council that some one of our Fellows should be appointed each year to prepare a Memorial Address to the memory and character of some deceased eminent surgeon or gynecologist, and that Dr. McMurtry be appointed to deliver such tribute to Dr. Ephraim McDowell at our next meeting.

Unanimously adopted.

The following papers were read by title:

1. "Contribution to the Study of Abdominal Pregnancy," by Dr. H. C. Coe, of New York.

2. "Tubal Pregnancy," by Dr. Joseph Price, of Philadelphia, Penna.

3. "Some Kidney Operations, with Remarks," by Dr. Geo. Ben. Johnston, of Richmond, Va.

4. "Poisoning by the Bite of the Southern Spider," by Dr. J. T. Wilson, of Sherman, Texas.

5. "Intestinal Anastomosis without Mechanical Devices," by Dr. John D. S. Davis, of Birmingham, Ala.

6. "Stone in the Bladder, with Report of Cases," by Dr. F. W. McRae, of Atlanta, Ga.

7. "Surgico-electrical Treatment of Epilepsy," by Dr. B. E. Hadra, of Galveston, Texas.

8. "Nature of Shock and Allied Conditions," by Dr. William C. Dabney, University of Virginia.

9. "The Part that Rectal Diseases Play in Women," by Dr. Joseph M. Mathews, of Louisville, Ky.

10. "Treatment of Stones in the Biliary Ducts," by Dr. W. E. B. Davis, of Birmingham, Ala.

11. "Case of Obscure Abdominal Hemorrhage," by Dr. I. S. Stone, of Washington, D. C.

12. "Laceration of the Cervix Uteri as a Factor in Producing Placenta Prævia," by Dr. F. Grange Simons, of Charleston, S. C.

13. "A Case of Cyst of the Broad Ligament," by Dr. D. Malloy Prince, of Laurinburgh, N. C.

14. "Abdominal Hysterectomy without a Pedicle," by Dr. Rufus B. Hall, of Cincinnati, Ohio.

15. "Traumatic Injuries of the Head," by Dr. John T. Chapman, of Bessemer, Ala.

16. "The Curette in Obstetric and Gynecological Practice," by Dr. John G. Cecil, of Louisville, Ky.

The election of officers was then proceeded with, the result being as follows:

President.—Dr. Bedford Brown, of Alexandria, Va.

First and Second Vice-Presidents.—Drs. Joseph Price, of Philadelphia, and George A. Baxter, of Chattanooga, Tenn.

Secretary.—Dr. W. E. B. Davis, of Birmingham, Ala.

Treasurer.—Dr. Hardin P. Cochrane, of Birmingham, Ala.

Council.—To fill vacancies: Drs. L. S. McMurtry and W. D. Haggard.

Place of Meeting.—New Orleans, Louisiana, second Tuesday in November, 1893.

Chairman of Committee of Arrangements.—Dr. Albert B. Miles, of New Orleans.

Dr. Brown, in accepting the Presidency of the Association, spoke as follows:

Mr. President and Gentlemen of the Southern Surgical and Gynecological Association: I cannot find words strong enough to express to you my gratitude for the high honor that you have conferred upon me and this mark of your confidence and esteem. It is peculiarly gratifying to me as coming from gentlemen with whom I have worked for five years. I believe this is a glorious institution, and God grant that it may prosper in the future as it has in the past. We rank with the first institutions of this kind in the world. If Mr. Bantock, Tait, Sir Joseph Lister, or Mr. Treves could have come to this country, and the office of President of this Association conferred upon them, they would consider it a distinguished honor, and perhaps merit it more than your servant occupying so humble a position in the profession. I love this Association. From its very incipiency it has been the opportunity of my life, of my happiness, and my pleasure. Five years ago a handful of surgeons, so to speak, assembled in the city of Birmingham to found the Southern Surgical and Gynecological Association. They determined that it should be a success, or they should fail in the enterprise. We started out right. We intrusted the honor of this high office to one of the most distinguished men in this country or any other country, and he led us on to success and to prosperity. We have been marching on ever since that time in glorious success.

Gentlemen, it is my daily prayer that God may bless you individually and as an Association. The only regret I have is that you did not confer this distinguished honor upon some one more competent than myself and better calculated to please you all. But my heart and soul and body are in the success of this Association, and as long as I live and am able to labor mentally and physically, my labors and time shall be devoted to it. I thank you, gentlemen.

A vote of thanks was tendered the retiring President for the able and impartial manner in which he had conducted the proceedings.

The President, in thanking the Association for its vote of thanks, stated that he had labored under a great disadvantage through a severe cough, and desired to thank his colleague, Dr. Kollock, for presiding at times.

Appropriate and timely speeches were then made by Drs. Cunningham, Love, and Palmer.

Dr. Haggard offered the following resolution, which was unanimously adopted:

Resolved, That the members of the Southern Surgical and Gynecological Association take pleasure in extending the expression of their high appreciation of the generous and cordial reception and royal hospitality extended them by the citizens and their brethren of the medical profession of the city of Louisville. They desire to make special recognition to the Committee of Arrangements for their admirable provision for their comfort and enjoyment, and its special outcome in the beautiful receptions tendered by Drs. Lewis S. McMurtry, Ap Morgan Vance, and W. O. Roberts, and the profession at large for their whole-souled welcome extended to the Association at the Galt House on the evening before adjournment. We desire also to place on record their appreciation of the courtesies of the press in its daily notices of the proceedings of the Association; their thanks to our President for the able manner in which he has presided over the meeting, and to Dr. George W. Griffiths for his kindness in securing the Council Chamber of the City Hall, in which the sessions of the Association have been held.

The Association then, on motion, adjourned, to meet in New Orleans, La., the second Tuesday in November, 1893.

W. E. B. DAVIS, M.D.,
Secretary.

ANNUAL ADDRESS OF THE PRESIDENT.

COMPATIBILITY OF CONSERVATIVE AND AGGRESSIVE SURGERY.

By J. McFadden Gaston, M.D.,
Atlanta, Ga.

The circumspect philosophy of former days taught us, What man has done man may do. But the developments of more recent times say, Whatever is practicable may be undertaken, without regard to precedents. Conservative and aggressive processes are combined in progressive surgery.

Conservatism in the use of all the appliances of surgery is not inconsistent with the application of the most energetic means of relief in structural disorders. A misapprehension exists with many of our profession as to the true sphere of progressive surgery, and it is my purpose on this occasion to make a distinction between rashness in the employment of operative measures and boldness in the use of surgical means of relief when clearly indicated. Real advances in surgical practice have not been the result of cutting and slashing without due consideration, but have accompanied painstaking investigation of the conditions requiring the knife, and caution in the performance of operations. As a preliminary to any surgical procedure of a radical nature, correct diagnosis is essential, but to acquire a proper understanding of a deep-seated

disorder it is often requisite to make an exploratory operation of greater or less magnitude.

The information based upon such an exploratory measure serves as a guide to any further surgical procedure. A verification of any complication, of a serious nature, by an exploratory step, may form a contra-indication for the more radical operation which has been contemplated. A great variety of means have been employed under the expectation of elucidating the doubts surrounding very obscure cases, some of which are of a precarious nature, proving far more hazardous than the disease or the proposed radical measure. It is evident that exploratory processes of this order should be ruled out of our surgical resources, and the prime consideration for our safe guidance in exploratory operations is to be sure that the existing disorders shall not be aggravated by such procedure, nor other graver developments be induced as a consequence of it. It is always best in cases involving mooted points to avoid the appearance of evil. In keeping with this precaution against the abuse of exploratory measures, it is proper to exercise great discrimination in the use of antiseptics in surgery. A most salutary result may be obtained by employing very active germicides in a case of septic contamination, whereas the same application may not be suited to an operation upon tissues in their normal state. It is now recognized by bacteriologists that certain preparations tend to destroy pyogenic organisms, and when they exist the use of such antiseptic means is indicated. On the other hand, it is becoming fully understood by clinical experience that a resort to these germicidal agents, when the pyogenic organisms are not present, operates unfavorably upon the healthy structure. The reaction in the practice of many surgeons of large practical experience, from observing the absolutely hurtful effects of the ordinary solutions of corrosive sublimate, when brought in contact with the absorbent surfaces of normal structures, is tending to a limitation of this poisonous

substance to external use. These do not belong to the class of "superfluous laggards" who are characterized as opposed to the methods of asepis and antisepsis, by the author of a recent work on surgery. But they are of that independent order of creation who are not bound down by any prescribed formulas and are bold enough to abandon a vicious practice which has brought trouble in its train to many victims of routine treatment.

It is well known to all surgeons who have investigated thoroughly the merits of antisepsis, that there are many germicidal applications far safer than corrosive sublimate, which meet all the requirements in surgery, and the day is not far distant when the use of the solutions of corrosive sublimate will be excluded from all operations upon incised healthy structures. There is a gradual movement of the pendulum of antisepsis toward asepsis, and it is found that sterilized water is the safest and best wash for surfaces not contaminated by previous septic developments in their tissues.

Antisepsis has a legitimate field of use in surgery, and it is to be regretted that the careless extravagance of the advocates of sublimate solutions should have given this process a black-eye by poisoning patients with them, who could have been treated successfully with other antiseptic agents.

Great detriment to proper antiseptic treatment has ensued from the indiscriminate application of solutions of corrosive sublimate, but judicious men are now more guarded in employing them in surgery.

Upon the general principle that no surgical operation of any magnitude should be undertaken without proper knowledge of the habits of life on the part of the patient, I may advert to the fact which is overlooked by many operators, in regard to the importance of continuing the use of any stimulant or narcotic to which the patient has long been accustomed. Within my experience the interruption even of the tobacco habit has been followed by

troublesome nervous depression, after operations, and the resumption of the use of this narcotic has afforded complete and prompt relief. I recall an instance of laparotomy for the removal of an immense cystic tumor of the ovary in a lady sixty years of age who, of her own accord, stopped smoking the pipe after the operation. For a few days all seemed to be doing well with the case, but at the end of a week there was a most profound depression of spirits, with vital prostration, which immediately disappeared upon resuming her pipe, and the final result was entirely satisfactory.

We all know that a patient who has been addicted to the use of alcoholic drinks for a considerable period before submitting to any surgical operation, cannot have it suddenly abstracted without being subjected to great disturbance of the entire nervous system. If he does not reach the point of perturbation that will produce delirium tremens, there will still be such derangement of all the functions as to interfere materially with proper nutrition, which may pave the way to serious complications in the after-treatment of the case. It is hence essential, under such circumstances, to take into account the previous habit in this respect, and continue to give certain quantities of the alcoholic stimulant at fixed periods to avert any troublesome consequences. An individual may fail to give such information as to habits of life in regard to the use of alcoholic stimulants unless specially interrogated, and yet would not hesitate to state the fact upon inquiry for the guidance of the surgeon in his case.

The most important matter, however, for our investigation, is in regard to the opium habit in one subjected to a grave operation. I am convinced from a case which has recently been reported to me, that operators of experience and with a full knowledge of the use of morphine by a patient, do not always realize the great detriment resulting from the sudden withdrawal of this article after an important operation. All abnormal conditions

resulting from any habit should be compensated in the after-treatment. Observation of the changes resulting from inflammatory processes should be accompanied by a study of those modifications impressed upon the tissues by impairment or undue activity of the nerve element which enters into their composition. That many operators fail to take into account the nervous system, in their surgical pathology, shows a lack of due consideration of the surroundings of a patient. While the legitimate field of surgery is the proper use of means of relief for organic or structural disorders, there are prerequisites in the recognition of the conditions warranting an operation and in the preparation of the patient for undergoing it safely, which should characterize the highest type of surgeon.

The distinction of external and internal treatment is held by most European authorities as the basis of recognition for the practice of surgery and medicine. But this line of distinction does not imply that all disorders of the inner structures are to be left for the treatment of the practitioners of medicine. Nor should all the pathological conditions not demanding operation be excluded from the domain of surgery. A proper recognition of the scope of surgery and medicine assigns to them respectively the organic and the functional disorders of the system. With this limitation of the province of the physician as contradistinguished from that of the surgeon, the work of the former should be confined to such measures as are calculated to correct the performance of the functions of the different organs of the body, whether internal or external.

When organic changes ensue, whether demanding the use of medication or a resort to operative measures, the case comes within the field of surgery. It presents a modification of structure, more or less pronounced, which alters the constituents so as to produce a departure from the normal state of the part involved, in its size, shape, or density. An incision or a puncture may be requisite to diagnosticate such a condition, but if this

change of structure could be clearly ascertained without any exploratory operation, the case would still fall within the domain of surgery. With this distinction, between functional and organic disorders for the practice of medicine and surgery, so soon as a change in the state of the case is recognized by the physician it should be transferred to the charge of a surgeon; and he will still have the disadvantage of combating the latter stages of organic disorders.

It may be that a skilful operator is not qualified for the highest attainment in surgery, from lack of proper precaution in proceeding with an operation.

The aim of the surgeon should be a due comprehension of abnormality in the structures of the part involved, and his end be to afford relief with the least possible injury to the organ or member which is the seat of disorder. Conservative surgery may be destructive of certain parts for the purpose of saving the structures, and the properly qualified surgeon should consider maturely every phase of the case under examination, so as to assume the responsibility of lopping off the disease and prevent its extension to the sound tissues. He who fails to use the knife or the cautery, when a resort to either would stay the progress of disease, is not the exponent of conservative surgery, but, on the contrary, aids and abets the work of destruction in the parts implicated, while he contributes ultimately to the death of the patient.

Ignorance and inexperience often lead to sad results in meddlesome surgery, when limbs are sacrificed or organs mutilated, to gratify the desire to figure as a bold operator on the part of a would-be surgeon.

In such cases no high-toned member of the profession should shield the culprit from the charge of malpractice or from the assessment of damages by a court of justice. While the allegation of malpractice, without sufficient cause, should never be encouraged by the better class of practitioners, it would be well to bring home to those who

rush into the surgical arena, unknowing and unknown, the consequences of their rashness. Of the two evils, over-caution of doing harm is far preferable to meddlesome surgery, and yet we would not hold him guiltless who stands with folded arms and suffers a patient to die who might be saved by a timely operation.

There is a field in surgery for masterly inactivity, and non-interferance is to be highly commended when by an operation a fatal result is precipitated. The want of a proper investigation of a case presenting grave complication may lead a physician to delay in calling a surgeon until the opportunity for relief has passed and his interference can avail nothing, so that he is justified in standing aloof.

There are numerous instances of the failure in surgical procedure from the late period of performing an operation, and it is greatly to be desired that a surgeon should be called in proper time by physicians having cases under their care which are likely to require surgical interference. If a consultation should be called and the surgeon found no indications for an operation, then, of course, the physician would be relieved of any future responsibility in proceeding with the treatment. But most probably their joint attendance would lead to the most satisfactory result in enabling them to determine upon the conditions developed in the progress of the case which might warrant an operation.

The dilatory spirit manifested by patients and those around them, as to resisting surgical means of relief in acute cases, has proved a barrier to the adoption of the expressed views of surgeons in favor of prompt action in many cases of great urgency. All practitioners of considerable experience have had occasion to regret that the opportunity for forestalling a grave malady has been lost by the indisposition to submit to a timely operation, and they have led a forlorn hope, to end in disaster.

In this connection, however, it may be appropriate to

consider the claims of a different case to our attention. When death is inevitable without an operation, and the knife affords the only chance, however slim that may be, for escape, it is a fair mode of dealing with this class of cases, for the surgeon to put himself in the place of the patient and determine what he would desire for himself under similar circumstances. Most of us, I think, in the possession of our faculties, would avail ourselves of the operation, and hope for the best result of it. Viewing matters from this standpoint, if those interested in the patient manifest a desire to take the risk of which all are apprised, I think we are warranted in operating even should the probabilities be greatly against a successful result.

It is very true that untoward results, even when expected, tend to discredit surgery and to give an excuse to others for declining an operation at a certain stage when it might serve a good purpose. But, considering the surroundings of the individual alone, we have certain death without an operation staring us in the face; and if the patient, after being advised of the situation, desires an operation, it should be performed.

But we have a serious question for settlement among ourselves as surgeons, and I am more especially concerned in the proper adjustment on this occasion of the differences in the surgical views of those who are equally entitled to think and to act in regard to surgical cases of great gravity.

In the course of consultation with other surgeons, I have sometimes submitted a definite view in favor of immediate operation, proceeding with a confident expectation of warding off a fatal result, when the response of my colleagues has been such as to discourage any interference. On some of these occasions the unfavorable verdict has been accompanied with the statement that, if I thought proper to take the responsibility of operation, they would render any assistance which might be desired, without, however, expecting any benefit. Of

course, I declined to rush into the breach under such untoward circumstances; and yet it has occurred to me that there ought to be some expression from the profession as to the line of duty for one who feels prompted to undertake the rescue of a patient from a most perilous situation, in which there is absolutely no hope without a surgical operation.

At the risk of being considered heterodox, I would draw the attention of the profession to a consideration of the propriety of immediate operation in that desperate class of cases which result from the crushing and mangling of limbs, attended with profound shock. It has been the custom of most surgeons to watch and wait while means are resorted to for restitution of the vital forces by stimulants. With our deficient knowledge of the exact etiological factor in this anomalous condition, I am inclined to the view that a continuous baleful influence is propagated to the nerve centres from the disintegration of the structures involved, and that this may be modified favorably by a clean incision through sound tissues above the point of injury very soon after such violence to the parts. It strikes me forcibly that surgical relief within fifteen or twenty minutes after an accident involving the muscles, bones, nerves, and bloodvessels, cannot intensify the shock, and that many of the cases left to die without any operation might be rescued by a prompt amputation of the member. The A.C.E. mixture offers the most favorable conditions for an anæsthetic and tends to lessen rather than increase the prostration, while the operation should be done with all possible despatch.

It may prove the most conservative surgery to lop off structures whose vitality is completely destroyed, and under the proposed state of the parts there can be no prospect of any restoration of nerve power or circulation to the tissues by delay in undertaking an operation. A proper appreciation of the participation in shock by the ganglionic nervous system, should impress the surgeon with the great importance of arresting the morbific influ-

ence at the very earliest period practicable, by removing the cause, while energetic correctives are employed.

At this point it may be appropriate to advert to the precautions requisite in all grave operative procedures to avert a depressing effect, independent of previous violence.

I have been convinced, by observation, that the use of moderate doses of quinine and strychnine during twenty-four hours preceding any important surgical operation, serves to ward off the nervous prostration. It has also been my custom to administer an alcoholic stimulant with a hypodermatic of morphine and atropine within a half-hour preceding an operation, and these preliminary measures have been attended with most satisfactory results. Prevention of shock is preferable to combating it by energetic means after a surgical operation.

As a fitting conclusion of this whole matter I would suggest that no surgical measure should be resorted to without looking into and correcting all underlying departures of the physical organization from the standard of health. It may not be possible to restore the normal condition of the secretions and excretions, yet means should be used for their correction, so far as practicable, as a preliminary step for even the most simple operation; as the surgeon never can know in advance what complications may be developed in the course of treatment.

To prepare a patient for undergoing any capital operation, when the forces of the vital organism have been exhausted by long suffering, it is requisite to support the system by tonics and nutritious food for days or weeks prior to the surgical procedure. A neglect of this precaution is inexcusable, except in cases of urgency, when the delay would be likely to aggravate the malady.

The after-treatment in surgical cases is in like manner of great importance to secure a good result, and this should include not only medication and food, but proper seclusion from exciting associations and due regard to the hygienic surroundings.

PERSONAL RECOLLECTIONS OF THE LATE DR. BENJAMIN W. DUDLEY, OF LEXINGTON, KY., AND OF HIS SURGICAL METHODS AND WORK.

By Bedford Brown, M.D.,
Alexandria, Va.

The surgical record of the great State of Kentucky is one that any people or nation could point to with emotions of pride and satisfaction. She gave to the world McDowell, the father of ovariotomy, and Dudley, the greatest lithotomist that this country has ever produced, and the most successful in the history of the world, and she can claim with all truth that the splendid reputation of Samuel D. Gross was nurtured on her soil, and in this her flourishing and beautiful metropolis. The history of our country is replete with the names of great and splendid men of genius, ability, and originality in our profession whose fame in past times was on the tongue of everyone in their day, but now are almost forgotten. Among those distinguished and illustrious surgeons of the past who belong to this class, the names of Physick, of Philadelphia; Dudley, of Lexington; Mott, of New York; Stone, of New Orleans; Eve, of Nashville, stand conspicuous for their skill, vast experience, and influence as operators and teachers of surgery.

These men were great leaders in our profession in their day, and controlled and directed surgical opinion throughout our land. They had not the lights of science to aid and guide them as we have in the present time, but had

largely to depend upon innate genius, practical good sense and energy for the attainment of their extraordinary success. They were strictly men of action, and in the midst of their busy, laborious lives found but little time to devote to contributions to medical literature. They were not only the most eminent and successful operators of their day, but men of profound thought and research, and the most learned and distinguished teachers in their respective schools of medicine throughout our land, from whose teachings thousands drew inspiration and knowledge. To the memory of one of these distinguished names, Dr. Benjamin W. Dudley, I desire to pay this humble tribute. It was my privilege to enter the office of Dr. Dudley as a private pupil in the year 1846, to remain until the conclusion of my medical education.

At that time there were two other office pupils—one a young gentleman from Virginia, the other Mr. Henry M. Skillman, now a distinguished physician of Lexington. My close relationship to Dr. Dudley as private pupil and assistant for two years, enables me to present to you a clear and faithful sketch of his character and surgical work. Though Dr. Dudley was at that time over sixty years of age, he was in the full possession of his faculties, mental and physical, and was in the zenith of his great reputation as a surgeon. For nearly a half-century he had had almost absolute control of the surgery of the West, not only as an operator, but his opinions on surgery held sway largely over the profession in that section. He was one of the founders of Transylvania University, and had occupied the chair of surgery throughout the existence of the institution for forty years. At the time that I entered the office of Dr. Dudley, Transylvania University was in a flourishing condition, and had an able and learned faculty. At that time the city of Lexington was regarded by universal consent as the Athens of the West, as a centre of classical, legal, and medical education, and of wealth and refinement. My first introduction to

Dr. Dudley, whom I had never seen, was under most interesting circumstances. My young friend, the student from Virginia, then a resident pupil, proposed to escort me for the purpose of introduction to my future preceptor to his private residence. As we approached the residence, we observed two gentlemen standing at the threshold; one was tall, commanding, graceful, and peculiarly imposing in appearance, whilst the other, though less imposing, was well calculated to attract attention. They stood with grasped hands, and in apparently earnest conversation. The one was the immortal Henry Clay, the greatest living orator on earth, the illustrious statesman, the adored and beloved leader, the pride and glory of Kentucky. The other was Benjamin W. Dudley, the famous surgeon of the West. Mr. Clay was just then on the eve of leaving the house of his devoted personal and political friend and physician. In his black broadcloth suit, swallow-tail coat, unvarying white necktie, silk hat, clean-shaven face, dignified demeanor, gray hair, Dr. Dudley might have been taken for a well-ordered, respectable clergyman. His personal appearance was the beau-ideal of neatness and cleanliness. But neatness and cleanliness did not end at the personal limits. They were principles in the practice of his profession that he held sacred and never to be departed from. In habit he was ever prompt and punctual; in manner courteous, polite, dignified, and positive. In movement and gait he was quick and decided. When going to an operation or visiting a patient his movements were so rapid it was difficult for his assistants and students to keep pace with him. He was frugal and abstemious in all things, and an uncompromising advocate of cleanliness and the use of pure water both internally and externally. He was fitted by nature, taste, education, boldness tempered with caution, fearlessness, manual dexterity, and penetrating powers of observation, to be a great and successful surgeon. Morally and physically he had no knowledge of fear. He had a

character of extraordinary firmness and decision. He knew no such thing as vacillation, and nothing daunted him. Difficulties that might appal others, to him only served to develop renewed courage and confidence.

Observing the gentle and pleasant smile that ever played on his placid countenance, one might have been misled into the impression that there lurked within a mild and gentle nature. On the contrary, his temper was quick, hot, and severe; his will imperious, strong, and absolute. Yet, with all, he possessed a kindly, noble, generous, and humane heart, that was ever ready to do charity and befriend the friendless. Patients with stone in the bladder, or other serious disease, would come to him from great distances, poor and in distress, and he would operate upon and cure them and send them on their way with their board bills paid without costing them a cent, but kindly words, rejoicing and blessing his name.

Yet with all his generosity and charity, through a long life of eighty-five years he had acquired wealth. It was said that the largest fee that he had ever received for the operation of lithotomy was five thousand dollars.

Dr. Dudley, after graduating in Pennsylvania University, in 1804, spent four years in the schools and hospitals of Paris and London, where he had the advantages of the teachings of the greatest surgeons of the times—Baron Larrey, Sir Astley Cooper, and Abernethy. From the teaching of the latter he imbibed his ideas of the constitutional origin of local diseases, which became a fixed principle with him that he applied to surgical practice during life.

Sir Astley Cooper he regarded as the beau-ideal of a surgical operator, and ever after took him as a model. As an operator, for cool, deliberate, calm self-possession, unfaltering courage under difficulties, precision and dexterity in handling the knife, and for extraordinary foresight, Dr. Dudley was unsurpassed in his day. We must

remember that all of his operations were performed without the advantages of anæsthesia, and amidst the heart-rending screams of his patient. Yet he was never thrown off his balance, and would remark: "Let them scream; it is a relief of nature." And notwithstanding the acute agony and suffering of his patient during operation, he rarely had one die on the table. Few men possessed more acute and clearer powers of observation, more penetrating foresight, more accurate views of the practical side of all surgical questions, or who had profited more richly by his personal experience, than Dudley. In many great questions, now regarded as well established, he was a half-century ahead of the profession. In the use of sterilized water as a dressing for wounds he had an abiding faith, and used it to the exclusion of all other dressings. Not a drop of non-sterilized water was permitted to touch a wound. He believed that unboiled well- or spring-water contained poisonous materials, the nature of which, of course, was not well defined in his mind. He contended that the purest form of water, next to the boiled or distilled, was cistern, which was used exclusively for drinking purposes in his family and office. His use of boiled water in all wounds, except those healing by first intention, was profuse and abundant. When dressing the wounds of his patients that were inflamed, or in which there was tendency to erysipelas, or when the circulation was feeble, the granulating process slow, or there was tendency to sloughing, or where suppuration was excessive, I have, in his presence and under his orders, poured boiled water, reduced to a temperature of 110°, on a wound or local disease for an hour at one sitting. And this process, if necessary, was to be repeated every four hours. The inspiring, cool, self-possessed manner, but always dignified and manly, of this great surgeon in the operating-room gave confidence and cheerfulness to all present. I never saw him hesitate or falter in an operation. His perfect knowledge of anatomy, acquired in the

great schools of Europe, gave him absolute command of his subject.

Preliminary to and after operation he practised a system of asepsis and antisepsis consisting of absolute regard for cleanliness in every detail. All extraneous dressings that might generate morbid influences were rigidly discarded. No lint, no ointments were permitted, but boiled water was applied in the simplest manner possible. He was an ardent believer in the truth of autogenesis, and, as far as possible, removed by diet and medicine all effete and poisonous matters from the blood through the great emunctories of the system. The eminent success attained by Dudley during his long and distinguished career as a surgeon was by no means due alone to dexterity as an operator; but to his thorough preparation of his patient and by placing him in a perfectly aseptic state by measures of cleanliness. These were controlling and guiding principles in his practice that were never departed from. While in those times bacteriology was a science unknown, and sepsis and antisepsis were things unheard of, Dudley understood the principles of asepsis, and he knew that all dirt and filth contained the seeds of disease, and to place his patient beyond the pale of disease was to preserve him in an absolute state of cleanliness. And furthermore, believing that the accumulation of effete matters in the system by errors of diet and torpid excretion generated a poison not less dangerous, he used means to eliminate as far as possible all poisonous effete matters from the body before operation. The general health of the patient was to be restored as nearly as possible to a standard of perfection.

Previous to operation, except in cases of emergency, the state of the patient's constitution was rigidly inspected. The state of the digestion, the character of the secretions of the liver, the intestines, kidneys, and skin; also the state of the pulse, tongue, and temperature, must be satisfactory. Thus the condition and action of every organ

was carefully scrutinized, and, if not up to the desired standard, they were to be placed in that condition by a well-regulated system of diet and medicine.

If the digestion was deranged or impaired, the functions of the liver and intestines defective or torpid, small doses of ipecac and calomel were given daily until the secretions were restored to a healthy state. He would rarely operate on a patient with a heavily coated tongue or with deranged digestion or torpid liver. The state of digestion, the secretions of the alimentary canal, and the assimilative functions were restored by means of a glass of hot water before meals; a half-pint of skimmed milk, brown bread, and corn-meal gruel at meals, while undergoing preparation for operation. During this preparatory course the patient was deprived of all stimulants, as coffee, tea, tobacco, or alcohol; but, in the event of much debility, he was allowed beef or mutton at dinner only. Dr. Dudley contended that a majority of people consumed too much animal food, and the redundancy consumed either went to waste, or accumulated in the system in the form of effete matter and poisonous material, and was often the means of producing disease of the liver and kidneys. Under his system of diet and medication patients who came to him broken down by disease, with loss of appetite and digestion, would often improve with astonishing rapidity. His preparatory system preliminary to operation resembled that now very much in vogue, and practised so successfully in the treatment of chronic disease at the famous Carlsbad, in Germany.

In his cases, if the renal functions were working badly, or the urine was loaded with phosphates or urates, uric acid, or mucus, his favorite correctives and diuretics were an abundance of boiled cistern water, warm lemonade, and corn-meal gruel. When these deposits existed, or there were indications of cystitis previous to operation for stone, the daily and free use of these simple diuretics would flush the secretory apparatus of the kidneys

copiously and clear up the urine, often in a surprising manner.

If the tongue, the alvine discharges, the complexion, the urine, indicated defective hepatic action and diminished biliary secretions, they were regulated by means of calomel and ipecac, corn-meal gruel well salted, and skimmed-milk diet.

Then, when the functions of the great vital organs had been restored to a healthy standard, the tongue made clean, the digestion good, the appetite keen, the pulse good, the patient was ready for the knife, and it was then applied promptly. And after his judgment was satisfied to operate and the operation was finished, we never thought of the possibility of unfavorable results, as suppurative fever, erysipelas, or gangrene. In surgical practice, Dudley had a great horror of the use of opium, and rarely used quinine. His materia medica was exceedingly simple, and was confined chiefly to the important eliminants.

Having described his preliminary management of his cases preparatory to operation, I will now proceed to describe his method of operating in special cases, his treatment of the patient after operation, and also his peculiar method of treating various surgical diseases. Lithotomy was his great and favorite operation. He never was in better trim or happier mood than when engaged in its performance. During his long life he performed the operation of lateral lithotomy two hundred and twenty-five times, with the loss of only three cases. Such unrivalled success, when published to the world by Prof. Petre, professor of chemistry in the Transylvania University, who had analyzed all of these stones, created in the profession of the Old World not only astonishment, but, on the part of many, incredulity until the facts were vouched for by other distinguished American surgeons. The only instruments used in performing these two hundred and twenty-

five operations were a scalpel, grooved staff, a Cline gorget, and forceps for grasping the stone and removing it. In his old mahogany case, manufactured in London and purchased there by him when a student, were scalpels, grooved staves, gorgets and forceps of different sizes, adapted to all ages from two years to old age. The old gorget of Cline, one of London's greatest surgeons in his day, few modern surgeons have seen. It is a curious instrument. It is made of a solid piece of fine steel, composed of a handle and blade all of steel without any extraneous material, and when dipped in hot water, which was always done, was really an aseptic instrument. The blade has a beak, a cutting edge, and back, the beak fitting in the groove of the staff. The steel handle is turned slightly to the right, which adapts it to the lateral operation.

After the patient had been conducted through the preparatory treatment and the general health restored, the digestion and appetite good, the secretions in proper condition, particularly the urine, the mental condition cheerful and hopeful, the general temperature and circulation normal, his entire body was carried through a thorough cleansing and renovating process by means of soap and water, in a warm bath. This cleansing process was something never to be neglected. In the operating-room he was laid on a table well covered with warm blankets, strapped in the lithotomy position, a grooved staff inserted in the bladder, and held by an assistant. Then the lateral incision was made by the scalpel down to the groove in the staff, the beak of the gorget inserted and rapidly plunged through the prostate into the bladder, giving vent to a sudden rush of urine; the finger of the left hand was then introduced into the bladder, and making a quick exploration, he introduced his forceps with the right, grasped the stone, and in an instant he had it in his left hand. He never enlarged the wound when too small, but dilated the opening in the prostate

while withdrawing the forceps with the stone in its grasp. After the operation the patient was placed on his left side for complete drainage, and only hot-water applications used on the wound.

In the centre of the floor of the old Medical Museum of the Transylvania University there stood a table with a glass case or cabinet containing all of these stones, labelled with the names of the patients, their ages, their residences, the date of operation and the results. All the patients I saw him operate on, varying in age from two to seventy-six years, recovered. Of the first hundred cases operated on by him, not a single death occurred.

His favorite day for performing important operations was Sunday, between 11 and 12 A.M., when most people were at church and quiet reigned in the streets. When he had decided to perform a great operation, in his brisk, quick, positive manner he would suddenly enter the office about 9 o'clock in the morning and say: "Young gentlemen, be ready to move to a certain house by the ringing of the last church bells." We always knew what this order meant, and all instruments and appliances were placed in readiness for his return, and just as the ringing of the last bells died away he would call at the door, and the little procession, consisting of himself, Prof. Bush, the demonstrator, Dr. Ethelbert L. Dudley, and his office pupils, would rapidly move to the scene of the operation, usually some hotel or boarding-house, and while the churches were resounding with the voices of praise a great operation was performed and a human life saved.

After all the complicated, tedious, and difficult methods of antisepsis practised for the past twenty years, the professional mind is just now beginning to revert to first principles and to adopt methods of simple, pure, unadulterated cleanliness as at least the best security against sepsis. Dudley was familiar with this fact seventy years

ago, and applied it in his daily practice. He was rarely more happy, even in the performance of his pet operation, than when drenching the wounds of his patients with boiled water, and it was with exquisite satisfaction that he watched the results on the circulation and the healing process.

Dudley approached nearer to perfection in the application of the bandage, and in its adaptation to the treatment of disease, than any other surgeon. The skill, the dexterity, the rapidity, the smoothness, the evenness of adaptation, every muscle being thoroughly compressed and controlled, while not a superficial artery was obstructed, was something to be truly admired. His guiding principle in its application was lightness, smoothness, and evenness over all parts where the main arteries were superficial, as the ankle, wrist, knee; but over the muscular portions of the limb to draw it as firmly as possible. The test was the detection of the pulsation of the superficial arteries through the bandage. He treated all gunshot wounds of the limbs by means of the compress and bandage with the purpose of healing them by first intention. The wound having been thoroughly cleansed with boiled water, a bandage was applied to the limb from the extremity up to the wound, then a compress dipped in boiling water was applied, generally an inch thick, over the tract of the wound; the bandage then was firmly applied over this compress, so as to compress together the walls of the wound, bring them into exact coaptation, and maintain them there until healed by first intention; and it was rarely that he failed in five or six days to accomplish the object. In those times the treatment of tetanus was universally by means of opium. We all know how signally this treatment failed to cure or alleviate the disease. Dudley took a new departure in its treatment. Relying upon the relaxing effect of tartar emetic upon the muscular, and its sedative action on the nervous, systems, he determined to test its action in the

treatment of tetanus. His peculiar method of using it in the treatment of this disease was to administer the drug in doses just sufficient to maintain the patient at the point of nausea, and to increase the quantity in proportion to the toleration of the patient without causing actual emesis, until all symptoms subsided. It was a severe and trying ordeal for the patient to be kept in a state of incessant nausea day and night for a period of five or six days. But he was often rewarded for his trials and sufferings by relief. During my pupilage I visited for him two cases of tetanus three times daily while under this treatment, and though both cases were well established, they recovered.

This treatment was maintained without relaxation day and night by faithful nurses to the end. Both of these cases were of the traumatic variety, and occurred in strong, robust men, from injury of the hand. He began treatment by giving an eighth of a grain every two hours, gradually increased until decided nausea resulted, and then the doses were either increased or diminished according to the indications of the case.

Dudley's method of treating stricture of the urethra was peculiar and entirely without instruments. After the first exploration with the sound to measure the extent, number, and character of the stricture, all instruments were laid aside. In his early professional life he had an unusually intractable and extensive case of stricture. This patient was attacked with typhoid fever of a very protracted character. After a confinement to the bed of three months, and having reached a degree of extreme emaciation, it was found that the patient could urinate without difficulty, when an exploration revealed the entire absorption of the stricture and its permanent cure. Subsequently he treated all his cases of stricture by absorption through reduction of the system by means of low diet and cathartic medicine to a point of emacia-

tion to which I should hesitate to hazard the life of my patients.

In practice we probably meet with two or three varieties of stricture: one, the old fibrous or cartilaginous form, consisting of an interstitial deposit of fibrous tissue having but little vascularity. This variety would be but little amenable to such treatment. The other forms are the congestive and inflammatory. These varieties of stricture we might well suppose would be affected by treatment such as would act on the nutritive and circulatory functions. And such was really the case in Dr. Dudley's practice, as I observed while attending his patients. Yet I confess that I would not like to take my patients through such a course of disease to cure stricture. But there can be no question that by this method Dudley cured a very considerable proportion of his cases, as his practice in this disease was large.

Except skimmed milk, the patient during this reducing process was denied all animal food and placed on brown bread and corn-meal gruel, while mercurial cathartics were used two or three times a week. This course was often maintained for weeks and months until the object was attained. Active exercise was also enjoined in the open air, and it was astonishing to observe what an amount of exercise those patients were capable of enduring.

In connection with this subject, I will say here that in my treatment of stricture the past twenty years, I have found in my own practice a combination method of treating stricture, consisting of a rigid system of diet, medicine, and mechanical dilatation, an improvement on either of the old methods. As Dudley taught, much animal food is deleterious to stricture. The only animal food allowed by me now in stricture under treatment is skimmed milk, and in addition brown bread and corn-meal mush or gruel. When the urine presents an acid reaction, I give the patient continuously the liquor

potassii citratis. When of an alkaline reaction, benzoic acid or diuretin. Then a systematic dilatation is practised by means of French olive-pointed bougies.

We have heard much in recent years of the surgery of traumatic epilepsy. Dudley, in this branch of surgery, was probably in this country the pioneer. He performed a number of operations on the cranium for the relief of epilepsy, more than sixty years ago. There were in all five cases operated on—three with perfect success, the others with partial relief. In the Transylvania Medical Museum could be found the circular portions of bone taken from the skulls of all these patients, with spiculæ of bone attached to the inner table from a half-inch to an inch in length, which had been removed with the trephine and was imbedded in the brain.

In the treatment of spinal curvatures and hip-joint diseases, Dudley discarded the use of all mechanical apparatus *in toto*, as being cumbersome and useless. He believed that the very beginning, the fountain-head of these affections, was located in imperfect digestion and mal-assimilation, and the imperfect formation of blood, and that the system failed to receive the nutriment necessary to sustain its reparative processes against local injuries; and furthermore, that if the blood was properly renovated and all mechanical pressure removed, the system would right itself and the local disease disappear. The patient was placed and maintained in absolute recumbency on a firm mattress during the entire treatment, and not permitted to assume the erect posture, but allowed to roll over his bed at will, and was amused in every possible way. The state of the digestive organs and general health received constant attention. The diet consisted of fresh meat once a day, skimmed milk, brown bread, corn-meal gruel three times a day. Under this regimen the digestion of these little patients would be restored, the appetite would become sharp, the secretions active, the complexion would lose its pallor and become rosy,

the mind playful and cheerful, and the local affection steadily progress toward restoration. Another feature of his treatment of these cases was daily massage practised over the entire body and limbs by means of towels saturated in salt water and dried. This practice gave a stimulus to the circulatory system that invigorated the general nutrition in a surprising degree. I visited daily five cases—two of hip-joint disease, three of spinal curvature—in children treated by him in this manner, and all made permanent recoveries. As mentioned in a previous page, Dr. Dudley believed that a great majority of local affections are the result of constitutional causes. In none were his peculiar views in this respect more clearly illustrated than in his treatment of erysipelas, traumatic and idiopathic. He believed that erysipelas was a localization of poisonous effete materials accumulating in the blood from defective assimilation and secretion. Acting upon these principles, his treatment consisted in eliminating these effete materials from the system through the great emunctories—the liver, intestines, kidneys, and skin —and the restoration of the blood by digestible diet. By means of calomel and ipecac the secretions of the liver and intestines were thoroughly regulated. The functions of the kidneys were kept in constant action by means of fresh citric acid and the skin by warm baths, while boiled water alone was applied every two or three hours to the local affection. I have seen repeatedly in his and my own practice aggravated cases of this disease recover rapidly under this treatment.

The method of treating fungus cerebri by Dr. Dudley was an exceedingly interesting and successful one. Over the protruding fungus, in injuries of the skull, a piece of soft linen was spread, and over this a soft, dry, compressed surgeon's sponge was bound with a bandage sufficiently firm to maintain it in position and not to force the fungus within the skull. This sponge gradually absorbed the secretions escaping from the

interior of the cranium and as gradually expanded, and acting as a graduated compress slowly and equally forced the growth back within the opening and kept it there until the wound healed. Following this method, no symptoms of cerebral compression ever resulted from this practice. In a case of my own, of extensive fracture of the frontal bones and loss of a larger portion of these bones with extensive injury and loss of the frontal lobes in the progress of the case, a large fungus sprang from the brain and was treated by this method with the most satisfactory results.

In regard to Dr. Dudley's judicious and practical discrimination between pure and impure water, so little considered in those times, and his knowledge of the fact that water constituted the principal carrier of morbid germs, he did not hold it merely as a theory but applied it in practice systematically.

For the purpose of carrying out this theory practically, he used for drinking purposes in his entire family, including office students and servants, a very pure and delicious cistern water exclusively. None other was permitted to enter his household. Connected with this fact, a very interesting incident occurred worthy of being recorded here, especially at this time. In the year 1832, Asiatic cholera of a most malignant type prevailed in Lexington, carrying off nearly a thousand victims in a population of eight thousand inhabitants. There were only two cisterns in the city, that of Dr. Dudley and his friend Dr. Bush. Not a single case occurred in either the family of himself or Prof. Bush, while the disease visited every other family in the city.

For many years the sole treatment of acute peritonitis was by means of opium and calomel. In all surgical diseases, and especially in this, Dudley entertained an insuperable objection to opium. It will sound strange in the ears of modern surgeons to learn that Dudley treated peritonitis more than a half-century ago by means of

cathartics alone—the present method of treating this disease.

His idea in rejecting the use of opium in the treatment of peritonitis and other surgical diseases was based upon rational grounds and principles. He believed that the effect of opium was to arrest all elimination and lock up in the system poisonous materials that, if retained, would surely cause fatal inflammation. Acting on this principle, he not only purged his patients when threatened or suffering with acute peritonitis, but he purged boldly and fearlessly. This afforded another instance for the practice of his favorite theory—the constitutional treatment of local diseases.

Dudley's method of treating hydrocele differed widely from the operation practised then by injection. With one sweep of the knife he laid the entire sac open, exposing the testicle, after washing the scrotum thoroughly. The open sac was then washed with boiled water, and a tent of linen cloth inserted into the cavity, three or four inches long and one inch in width, which was permitted to remain for a period of three days in the sac, when active adhesive inflammation would be developed in the vaginal tunic, and the sac would become permanently obliterated from adhesion of its walls. Accompanying this adhesive inflammation, very considerable swelling and œdema of the scrotum would arise. This was treated successfully by means of repeated hot-water douches to the inflamed organs.

In my own operations for hydrocele I have followed closely the method of Dudley in laying open the scrotum and vaginal tunic, evacuating the fluid, washing out the sac with boiled water; but for some years past, previous to inserting the linen tent, have swabbed out the sac thoroughly with carbolic acid, and then introduced the tent, which was permitted to remain for three days and then removed, when I have invariably found that adhesion had taken place, and the sac was completely obliterated.

Much is being said at present in regard to the radical treatment of hernia of long standing, and as Dr. Dudley's method of treating this disease differed materially from those in common resorted to, I deem it worthy of mention here, because of the high degree of success resulting from his operations in effecting permanent cure. After the hernial protrusion had been exposed by incision, the hernial sac opened, and the hernia reduced, the sac was permitted to remain, the wound was thoroughly washed with boiled water, and then charpie or lint, saturated with boiled water, was packed in the wound and secured by means of compress and bandage, and removed every day after a thorough douching with hot water. This process was continued from day to day until the wound was perfectly healed or filled up by the growth of granulation. The sac and abdominal ring were so thoroughly closed by this procedure as to render all future hernial protrusion impossible.

In evidence of the great ability of this remarkable man, of his vigorous common-sense views, and his practical management of all surgical questions and methods, and of his great success, I might add much more, but I feel sure that sufficient has been said in vindication of his great name, of his immortal work, and of his cherished memory, to enable those of the present generation to realize the fact that in our profession in the past generations there were gigantic intellects, men whose fame was as widespread and whose skill with the lights before them was equal to the most brilliant lights of the present day.

EXPERIENCES IN PELVIC SURGERY.

By A. V. L. Brokaw, M D,
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Of all the surgical problems difficult to solve it may be truthfully said that those met with in the pelvis are the most trying. I know of no surgical work which can compare with the experiences found in the pelvis: such a diversity of conditions, complications, and unexpected happenings are ever presenting.

In a series of many operations but few will be alike in every particular. Uncertainty of making an absolutely correct diagnosis before operations enters largely into the experience of all operators in this field. When I had little experience in this work, I could always make a diagnosis, and quite frequently verify it after operating. With an increasing experience I find myself well satisfied with an approximate diagnosis.

The diagnosis of pus cases is usually clear, and one makes a correct diagnosis in the majority of cases. It is in the other pelvic conditions where we make our errors of diagnosis. We might very properly state that only those without operative experience, or the theorist, always make correct analyses of their cases. Individually, I frankly admit my inability to correctly diagnose the character of abdominal and pelvic troubles as my experience becomes larger. I have diagnosed pus tubes, and found extra-uterine pregnancy; diagnosed extra-uterine pregnancy, and found pus; diagnosed ovarian lesions, and found the trouble located in the tubes, and *vice versa*.

This experience tallies with that of all the most prominent workers in this domain of surgery with whom I have conversed.

The bad results in pelvic work are frequently found to be the result of neglect, often ignorance, and very frequently a want of resolution in the attendant. An early use of the knife in many cases which eventually pass into our hands, would have given far more brilliant results and improved generally the present statistics in pelvic surgery. The number of cases we see in the early stages of disease are relatively infrequent; by far the greater number are in a late stage of disease; not a few on the verge of dissolution.

The criticism passed upon pelvic surgeons, that they often operate when operative proceedings are unnecessary, is usually unjust, and simply the gratuitous statement, actuated by bad motives, of those without personal experience. No conscientious, earnest surgeon has a thought but for the best interest of those placed under his charge.

When well-defined pelvic lesions exist, nothing short of radical measures succeed. Abortive attempts at dealing with pus or other threatening conditions only bring disastrous results. I am a believer in the conservatism so well defined by McMurtry in a recent paper, and I heartily recommend the perusal of that erudite article to the critics who never operate.

That needless, reckless surgical exercises are perpetrated in the pelvic work of a few we will not attempt to deny; however, the same is true in other directions, particularly so in the cerebro-spinal surgery which recently threatened to become epidemic.

The routine gynecologists are largely responsible for much of our work. By a routine gynecologist I refer to the gynecologist who treats all his cases alike: introduces a speculum, paints the cervix with iodine, makes local applications to the cervical canal or within the uterus

itself, insufflates some boric acid, introduces a glycerin tampon or pessary, and requests the patient to come again in two days. This soft impeachment cannot be denied.

The specialist should recognize conditions demanding imperatively operative interference, and not nurse cases along in the described manner until they become "wrecks," wretched examples of what electricity, the sound, dilator, and cautery can do for women.

What I have said in these paragraphs does not apply to all specialists in women's diseases, but, unfortunately for the other sex, it does apply to the majority. Much of my work comes to me after all the methods of minor "gynecological tinkerings" have been exhausted. The most difficult "pus" operations I have performed have followed prolonged treatment at the hands of gynecologists, or, better, the pseudo-gynecologists. An example of the character of the cases one meets with after meddlesome manipulation with the sound will be *apropos:*

Mrs. C., suffering with dysmenorrhœa of some severity, was recommended to a local specialist, who told her "that the mouth of her womb was closed up," and it would be necessary to dilate it at once, which operation he immediately set about to perform. A uterine sound was introduced. This caused no little pain, but she was assured that was to be expected. At midnight her attendant was summoned in haste, to see his patient suffering excruciatingly. A high temperature and great abdominal tenderness were present, and then for six weeks a severe form of peritonitis was combated. As a result of the opium treatment of the peritonitis, the patient became addicted to morphine, was treated by various local neurologists; wound up in Philadelphia, temporarily cured of the habit by Dr. Mitchell. While in the East was examined by our distinguished friend, Dr. Barton Cooke Hirst, who advised operation for the well-marked tubular disease and abscess of the ovaries. The case was referred to me for operation after her return, St. Louis being her home. Operation revealed universal adhesions, multiple pelvic abscesses, ovaries and tubes containing several ounces of pus. The adhesions were

dense, and the most difficult to deal with that I have ever met. The serous coat of the neighboring intestines was stripped off in many places in the process of enucleation. After a free flushing, a tube was introduced and my troubles began. The morphine mania returned, and the patient became uncontrollable and insisted on having morphine whenever she desired, in spite of earnest entreaty. From then on she took full charge of her case, and the nurses were unable to manage her. The tube began to discharge fecal matter on the third night after the operation and continued to do so for seventeen days, when it suddenly ceased, the stercoraceous fistula closing rapidly.

I might detail other cases which have come under my observation demanding operative interference, where the inflammatory conditions were the result of the introduction of the sound, caustics, etc.

But a few weeks since it was necessary to operate in an emergency on a leaky pus tube which had its origin in a similar manner. The original tubal disease had been treated with an intra-uterine grooved stem pessary, the idea of the attendant being that drainage *per viam naturalis* would follow. Vaginal incision is a favorite method of treatment of pelvic pathological collections with some. In my experience, but few, if any, of these cases can be considered properly or perfectly cured by such means.

We find multiple abscesses in the largest proportion of cases which cannot be drained by efforts so blindly and indirectly made. The abscesses may or may not be communicate with one another, and usually only one such abscess would be drained by a single vaginal incision.

An intelligent practitioner recently requested me to see his wife, who had been having recurrent attacks of peritonitis. She had undergone a vaginal incision eighteen months before her present attack of peritonitis. Was relieved for a few months, but latterly had been losing ground on account of numerous attacks of peritonitis. I found the pelvis choked up with inflammatory masses, a fixed uterus, and at the site of the old incision a soft

bulging point in the otherwise board-like vaginal vault. Her husband wished me to open the threatening abscess, which I refused to do as a matter of principle. The abdomen was distended, with signs of an aggressive peritonitis present. A temperature of 103° and over had existed for two weeks prior to my visit. I suggested a section, which was not agreed to. A few days later I was summoned in haste. All the symptoms had become aggravated and her condition, indeed, alarming. Consent was given to an operation. The peritoneum was thickened; omentum agglutinated in every direction; matted intestines; a pint of pus in the pelvis; multiple abscesses surrounding the uterus on all sides. An abscess the size of an egg, isolated from the other pus collections, proved to be the one pointing into the vagina. The abdomen was flushed, tubes inserted, and my late evening visit found a normal temperature, and recovery from then on was uneventful.

Had I been persuaded to make the vaginal incision, the register would have rung up another surgical failure.

An experience in extra-uterine pregnancy forces upon me the conviction of the wisdom of Tait's law of exploratory incisions in obscure disease of the pelvis and abdomen. The force of his arguments cannot but grow in favor and importance in the estimation of anyone with surgical instincts who desires to do what is best for his patient.

The one condition above all others where exploratory incision should be adopted is in cases of suspected extra-uterine pregnancy. It is correct and good surgery to open the abdomen, and not wait for all the classical signs to appear. They may come, it is true, and your patient may be lost by procrastination. It may not meet with the kind accord of this body of distinguished specialists, but it is my belief that we should, in all cases of suspected extra-uterine pregnancy, do an exploratory operation—a short, clean incision. If nothing is found, you have at least relieved your mind of anxiety born of the fearful experience such cases only can give. What surgeon hesitates to operate in accessible aneurisms? Yet in extra-uterine pregnancy

do we not have a condition equally as bad? The text-books advise that electricity and other measures be tried first, and operative procedures later. The results of such teaching we read in the mortuary columns of the daily press.

The symptoms of extra-uterine pregnancy are so frequently obscure and unreliable that I am firmly convinced that the radical position taken is correct.

An example of an unusual history in a case of extra-uterine pregnancy is now under my care, operated upon three weeks ago:

Mrs. T., at twenty-eight, the mother of four children, desirous of no further increase in her family, having missed one period, on advice of a neighbor took repeatedly large doses of oil of tansy; was seized with flooding, and supposed she had aborted. Hemorrhage became quite profuse. After several days called in a physician. "Had some fever and slight chills." The discharge was offensive, and the womb was curetted with a blunt curette and a few clots removed. Fever did not abate, but rapidly rose; great tenderness; symptoms of peritonitis rapidly developed. Seen by me the night of the eleventh day. Temperature 105°; surroundings were bad, and patient was sent to the hospital at once. Operation early the following morning. Appearance of patient peculiarly bad, pulse weak. Section; abdomen filled with blood; extra-uterine pregnancy; right side. Patient's condition on table alarming—almost pulseless when abdomen was opened. Tubes rapidly enucleated; tied off; free saline irrigation; drainage-tube placed in position; usual treatment of impending collapse instituted. Reaction very gradual, but followed by complete recovery, temperature never reaching above 101°; normal after the first week.

My faith in the dorsal position, short incisions, the drainage-tube in nearly every case, and free use of hot water remains, and I do not see the advantage, if any, of a departure from such technique.

DISCUSSION.

Dr. William Warren Potter, of Buffalo.—Mr. President, though you have taken me somewhat by surprise in inviting me so kindly to open the discussion on this paper, and as a consequence I have not formulated any special line of argument, yet I recognize the fact that it is a paper deserving of a prominent place in the proceedings of this Association, and ought to have that further recognition which is accorded to valuable papers, by thorough discussion.

Dr. Brokaw has given groundwork for a discussion upon several important subjects, but I will now only allude to two. He has made some statements with reference to the uterine sound with which I find myself in accord. It was my privilege, as well as pleasure—and I then felt it a duty—to bring an indictment some six or eight years ago against the uterine sound. That indictment, singularly enough, was brought to the city of St. Louis, the home of the author of the paper under discussion, and I am glad to hear him repeat in this presence to-day an approval of many of the principles that I then sought to enforce. At the time they were uttered they were accorded either condemnation or but faint praise. They were by some regarded as false teaching, by others as somewhat in advance of the time in which they were uttered. From the tone of this paper, as well as other reports that are occasionally heard, I infer that the time has crept up close to the position that I then took, and I especially commend what Dr. Brokaw says on the subject.

The paper, indeed, as a whole, demands very little antagonism or adverse criticism from anyone who has had personal experiences in the practice of pelvic surgery. I particularly commend that portion of it which relates to an early exploratory incision in suspected cases of extra-uterine pregnancy, or where there is made a presumptive diagnosis of that condition. This is one of the most important questions connected with abdominal surgery at the present time, because it is of frequent occurrence, is so often overlooked, and so seldom proceeds to other than fatal termination unless a prompt operation is made. It is one of the easiest operations, ordinarily speaking, in the whole range of

abdominal surgery: incision, turning out clots, tying off, irrigation, drainage, and closure, being the several steps of the operation that are to be promptly and understandingly taken by a surgeon of experience. The principles involved are quite the same as enter into a case of crushing, or other injury, of the extremities from railway or gunshot accidents. Here, there is frequently hemorrhage and shock to contend with; and so, likewise, is there in cases of ectopic pregnancy. How there can be a question as to the propriety of prompt surgical interference, it is not easy for me to understand. But I have already exceeded the limits I had laid out for myself, when I arose simply to speak of these points made by the essayist, and to commend his paper for its intelligent presentation of them.

Dr. Joseph Taber Johnson, of Washington, D. C.—Dr. Brokaw has given us a very suggestive and interesting paper. I agree with Dr. Potter that it is a pity to allow the excellent points he has brought out to go by without discussion. The points made in his paper were so numerous that it is impossible to take them up *seriatim* in the five minutes allotted to each speaker. I want to say a word about the question of diagnosis.

I have known a number of cases in abdominal surgery to have been spoiled for the abdominal surgeon on account of the delay for the purpose of making an accurate diagnosis. It seems to me on this question, that the ground which we need to take is, that when we have diagnosed something in the abdominal cavity which ought not to be there, anatomically or physiologically, and is histologically wrong, threatening life, it should come out. If we could reach that conclusion scientifically, it would be a good thing. I think an exploratory operation is justifiable, and we should base our further procedures on what we find after making the exploration.

So far as the author's statements are concerned in regard to neglected cases, I agree with him in his experience. The operator is often hampered and his operation jeopardized in many cases, and patients are absolutely lost by the neglect and delay of those who previously had charge of the case, or the surgeon's own lack of courage in carrying out his convictions.

I agree with the essayist, also, that the critics of pelvic and abdominal operations are in a great many cases men who have

never operated, and who, being without experience, have no right to criticise. So far as tinkering is concerned, I do not quite agree with the Doctor that it is as bad as he would have us believe, and that the routine gynecologist is given over entirely to the use of the speculum, nitrate of silver, tincture of iodine, etc. The class of men who practise gynecology in my vicinity have a wider range of appreciation of these troubles than the paper would seem to indicate, hence I feel that he has rather overstated the matter in that respect. I do not go quite so far as Drs. Price, Potter, or Brokaw, and say that the uterine sound should never be used. I think it is a good instrument in some cases. I think that while whiskey and opium have done considerable harm, still there is some good in them when properly used; I think the same may be said of the sound. While I may have done harm with the sound, I am positive I have done much good with it. In the operation for lacerated cervix, curetting the diseased glands of the cervical canal, and curetting after dilatation of the cervix for the purpose of curing endometritis, I think a great advance has been made on the old method of treating the latter condition especially, and *this* kind of tinkering, if it is properly termed so, has resulted in much good in the treatment of this disease, which has heretofore taken months, if not years, to cure.

If you have an ovarian or tubal abscess divided into compartments, as so frequently occurs, you cannot puncture through the vagina and poke your instrument in various directions to find pus. It seems to me that the woman's condition is better in the long run, and safer in a large number of cases, if we do an abdominal operation rather than vaginal puncture. When you have emptied one cavity you may have a pyogenic membrane left, one cavity is filling up while the other is emptied by operation. The Doctor has alluded to operations with high temperature. Some surgeons are frightened by the bad condition of the patient and the alarm of her friends. I have seen patients rescued from death by a proper operation with a temperature of 105° and a pulse of 140.

I quite agree with the writer of the paper that we should make exploratory operations more frequently, but we should stop when we find the case incurable before having done more harm than simply to explore. Half-finished operations are more fatal than

when completed. I have seen much unexpected good result from a mere exploration when it was considered too dangerous to proceed, but only disaster from abandoned incomplete operations.

Ruptured or unruptured tubal pregnancies should always be operated on as soon as diagnosed. There is no safety in delay, and electricity is too uncertain to rely upon. Even when it kills the fœtus the woman is not cured and is exposed to many dangers.

Dr. W. E. B. Davis, of Birmingham, Ala.—It would be difficult to discuss Dr. Brokaw's paper intelligently without covering the whole field of pelvic surgery. I believe that a great many of the so-called "tinkerers" who succeed in relieving their patients, do not accomplish it so much by the local treatment they use as by having the women under their care, keeping the bowels open, and seeing them regularly. It is not the glycerin tampon and boric acid that do all the good, but the patients are benefited by coming to the physician regularly, and being instructed as to diet, exercise, etc. Thus the mild cases of pelvic trouble are frequently relieved. There is no question as to what should be done in severe tubal or ovarian inflammations. The profession is agreed that in these cases nothing will succeed but a section, and the removal of the diseased organs.

In regard to diagnosis, I fully agree with Dr. Brokaw, that it is generally the men who do not open the abdomen who can confidently diagnose these cases. Those surgeons who are opening the abdomen constantly are so frequently mistaken, that they are rarely willing to give a positive diagnosis. I have seen a small cyst in the abdomen where there had been an attack of peritonitis, and not having seen the case before, when called, I thought it was an abscess. I have made the mistake twice within the last year. It is exceedingly difficult sometimes to diagnose pelvic growths, and especially ectopic cases.

I believe that the teachings we get from Dr. Price are good, still I think he goes too far in saying that an endometritis is a small matter. His views are extreme in this regard. I think where we have an endometritis, that by relieving it we prevent tubal trouble, and thus avoid an operation in the future. Many of our tubal diseases are due to neglected cases of endometritis, and there can be no doubt as to the value of the treatment

that has been practised so largely recently, of curetting the uterus and packing it with gauze. The trouble is, a great many cases fall into the hands of incompetent practitioners who abuse the operation.

Dr. Brokaw.—I wish to emphasize the point in regard to uncertainty of diagnosis. Recently a writer in New York, with an experience of one case of extra-uterine pregnancy, felt called upon to write an article on the subject, and tells us we should diagnose our cases of extra-uterine pregnancy early, wait until rupture before operating, and even then, wait till reaction takes place. We find gentlemen from the East writing upon this subject, giving the means by which they diagnose extra-uterine pregnancy before rupture. If there is a man in the East who diagnoses these cases always, I am pretty sure that he has some method of diagnosis unknown to us. I have seen these cases before and after rupture when unsuspected. I have made this mistake in two supposed pus cases, and I found tubal pregnancy. We do not often make a diagnosis before rupture. Whenever we have a suspicion of extra-uterine pregnancy, I believe it is good surgery to open the abdomen at once. I have never regretted the cases I have explored and found nothing. My mind was relieved. I have never lost an exploratory case. The operation is not done often enough. I believe in every case of suspected extra-uterine pregnancy we ought to make an exploratory incision and operate before rupture.

CRANIOTOMY UPON THE LIVING FŒTUS IS NOT JUSTIFIABLE.

By Cornelius Kollock, M.D.,
Cheraw, S. C.

Craniotomy is, doubtless, the oldest of all obstetric operations, and the hook and perforator antedate all instruments ever used in obstetrics. The operation, of course, implies the death of the fœtus and a frightful mutilation of its body, often accompanied by serious lacerations of the vagina and adjacent tissues of the mother. All this is done solely for the protection of the life of the mother. When there is absolute certainty that the fœtus is dead, craniotomy is a justifiable operation; but even then there are circumstances under which its propriety may be questioned, or it may be positively forbidden. A contraction of the pelvis so great as to reduce the measurement of the conjugate diameter to one inch and a fourth, as sometimes happens, positively forbids an attempt at craniotomy, for the space in which the instruments must be used is so narrow as to render the work impossible without great laceration and mangling of the parts, involving sometimes the bladder and intestines.

I was once called in consultation to a case in which craniotomy had been done. Only portions of the fœtus had been removed. Upon making a digital examination, also using the pelvimeter, I found the patient with a conjugate diameter measuring one inch and three-eighths; fecal matter was passing from the vagina through a laceration of the intestines; the bladder was also torn,

and there was a constant dribbling of urine. The physician in attendance on this case was a man of more than ordinary intelligence and large experience. His weakness consisted in his inveterate prejudice against the use of the knife, more especially against the abdominal section. This would have given the woman a very fair chance for life, and in all probability have saved the fœtus. Although she had not been in labor long enough to exhaust her strength, she died very soon from hemorrhage and shock.

This extreme case happened a number of years ago, at a period in my professional career when I was not entirely ready to denounce craniotomy or to embrace laparohysterotomy; still, it made a deep impression upon me, and set me to thinking how the murder of helpless infants could be avoided. I did craniotomy but once after this, and, thank God, it was upon a fœtus that had been dead several hours when I was called to the case. Craniotomy is done solely for the protection of the life of the mother, yet it often destroys the life which it proposes to save. We have no positive proof that Hippocrates, Celsus, or the Arabian physicians destroyed the life of the fœtus, but the operation was considered justifiable in their day. And when we consider how many women would have died without it, we may appreciate in a measure the tenacity with which it has been upheld. The mortality of mothers was greater in times past than now, but since Baudelocque, Hemming, Jones, Churchill, and Tyler Smith rate it at 40 or 50 per cent., the results of craniotomy cannot be considered satisfactory. Yet, if we investigate the subject more fully and in chronological order, we shall see that, while the mortality of mothers is less now than at any former period, owing to the advance in obstetric science and the more skilful performance of the operation, the number of fœticides is greatly increased. This fact may be, in part, explained by assuming that the more dextrous one becomes in craniotomy the more disposed he is to resort to it to avoid the

risk of waiting too long. While we hope and believe there are but few in the profession who would resort to craniotomy for a foul and wicked purpose, we must fear there are some who do the operation ostensibly to save the woman's life, but really to protect her against the troubles of maternity, or to shield her character from shame.

The flat rickety pelvis is the most common cause that renders a natural delivery of the fœtus impossible. Prof. Isaac E. Taylor, in an able paper in the *American Journal of Obstetrics*, proves that in all cases of deformed pelvis in which the contraction is so great as to reduce the measurement of the antero-posterior diameter to one and a half inches, craniotomy is not practicable. Perry, in the *American Journal of the Medical Sciences*, goes even farther. He contends that, in a pelvic contraction with a conjugate diameter of two and a half inches, the results of craniotomy are no better than those of the Cæsarean section—not as good. He might easily say more, for where the pelvis is so contracted and gives so little space to work in, the mangling of the parts excites a septic influence. There are cases of obstructed pelvis caused by rickets or by exostotic growth on the sacrum, or other points in the pelvic cavity, in which it would be wrong—criminal—to attempt craniotomy on the living or the dead fœtus. The severe treatment required, superadded to the exhausted condition of the woman, would very materially lessen her chances of recovery. All operations of craniotomy are after the labor has been in progress thirty-six or forty-eight hours, sometimes longer. Long-continued contractions of the uterine muscles and frequent manipulation of the parts, in efforts to turn or apply forceps, produce contusion which excites a septic influence and prevents union by first intention.

What, then, is our duty with the light before us? Are we to hold to the barbarous, murderous practice of craniotomy, because of its antiquity? Are we to allow con-

servatism to drift us into criminal old fogyism! In addition to the opinion of men of brain and experience, we have valuable statistics of the present time that should banish from the mind the thought that craniotomy on the living fœtus is a justifiable operation.

Recent advances in obstetrics, gynecology, and abdominal surgery contribute largely to a demonstration of the fact that a timely resort to surgical interference in pelvic obstruction is the great factor to success. Not long since, in Germany, out of 149 cases of contracted pelvis, 109 women and 136 children were saved, a preservation of 245 lives. If craniotomy had been done in those cases, 149 children would have been destroyed and probably fifty women—perhaps more—making a sacrifice of at least 199 lives. In many of these cases exhaustion had supervened and septic influence had already been excited. This, added to a tardy disposition to union by first intention, caused by contusion of the parts involved in the uterine incision, lessened materially the women's chances for recovery. Zweifel was successful in 29 cases out of 30. Schauta did Cæsarean section fifteen times without a single death. Schauta and Zweifel's operations were done in an early stage of the labor, before mangling and contusion of the tissues had excited a septic influence and before the strength of the women was exhausted. There was a time when the results of the Cæsarean section with the American surgeons were not equal to those on the continent across the water. But in the last few years a great change for the better has been seen in hospitals and in private practice. Recently, in 18 operations performed in Louisiana, 14 were successful. Of 8 in Ohio, 6 were successful. In December, 1891, Dr. Charles Jewett, at the Long Island College Hospital, did Sänger's improved Cæsarean section twice successfully—saved both mothers and infants. Dr. Howard A. Kelly, of Baltimore, has recently done the operation successfully four times. Dr. H. C. Coe, of New York, has done it twice successfully

within a very short time. Dr. Joseph Price, of Philadelphia, has done Cæsarean section a number of times successfully. Dr. Price prefers Porro's operation. There are many who prefer Sänger's improved Cæsarean, while others prefer Porro's. We will not at present attempt an estimate of the comparative merits of the two operations, for it is not easy to say to which the preference should be given.

While the literature of Cæsarean section is voluminous, it would be presumptuous in one whose experience is so limited as mine to attempt to present anything new. Dr. R. P. Harris, who has made Cæsarean section a special study, and has collected more valuable statistics on the subject than, perhaps, any man living, is able to give an opinion of value and one that must attract the attention and command the respect of all. Dr. Harris considers the two elements of success to be, an early operation and perfect technique. Would it not be well to add, as a third element of success, a quick performance of the operation? The shock in all abdominal operations, especially in Cæsarean section, is always increased by too long access of atmospheric air to the cavity. In all the abdominal sections I have ever done or witnessed, I have always observed that the longer the cavity was kept open the more unfavorable was the prognosis.

A case of pelvic contraction in which I operated successfully a short time ago, confirmed me in the belief that the sheet-anchor of success is an early operation. Let the surgical work commence before the woman is bereft of strength, and before the parts are in a condition to take on unhealthy action. In this case I bore in mind the importance of rapid work and kept the cavity exposed as short a time as possible.

The operation was done March 20, 1892. Patient, aged twenty-eight years, had been in labor about three hours when I first saw her; general health was apparently good, but there

was a severe pelvic contraction, consequent upon some diseased condition of the bony structure of the pelvis. She had had craniotomy done twice—in July, 1885, and in September, 1887. The parts were very much mangled each time—so much so in the second labor that she came near losing her life. When I was called to her, the attending physicians, both intelligent men, informed me that they had destroyed two children for her, and for that reason declined a third croniotomy. They also said they thought the contraction greater than in the previous labor. In this they were probably correct. I made a careful exploration of the pelvic cavity, and found the conjugate diameter measuring not more than one inch and a fourth. I refused to do craniotomy, and proposed the Cæsarean section, which was readily consented to by the patient and her family, when they were told that she could probably stand the operation, and that it would give the child a fair chance of life. She said that if another child must be sacrificed she would go with it. I did Sänger's improved Cæsarean section and was as rapid in the work as I dared to be. The operation consumed just forty-four minutes. The patient rallied from the shock as soon as the effect of chloroform wore off. I never had a case to do better after a far more simple operation. The pulse was never more than 95, and the temperature never above 98°. There was perfect union of the incision by first intention, and the sutures were removed on the tenth day. Mother and child, at the end of seven months after the operation, are doing finely.

Is there any special reason why the Cæsarean section should be more fatal than laparotomy for the removal of ovarian cystoma or other pelvic growths, when performed under proper circumstances and by skilled and experienced hands?

The Cæsarean section, if historic accounts are true—and we have no reason to doubt them—is nothing new. It was probably done many centuries ago, and it has been done occasionally during the last few centuries. According to Dr. Harris, the uncivilized inhabitants of Uganda, in the heart of Africa, have been in the habit of performing it. This fact was brought to light by Robert W.

Felkin, of Scotland, who saw it done successfully by a native operator. And women have been known to do it on themselves.

From present data we have no means of estimating the antiquity of the operation. The fact that the African laparotomist washes his hands and instruments and bathes the abdomen of the patient with palm wine, such as was used by embalmers in ancient times, suggests that he may have some vague and indefinite idea of asepsis. This idea may, perhaps, be traced back traditionally to the period when the bodies of the Pharaohs and other distinguished Egyptians were embalmed for deposit in the Pyramids.

These people, ignorant not only of anatomy, gynecology, and abdominal surgery, but of everything that would seem to fit them for such work, go readily into this severe capital operation and perform it successfully, while many of the learned and skilful surgeons of our own age and country shrink from it and resort to the barbarous act of destroying the innocent fœtus while imperilling the life of the mother.

It is my honest conviction that 85 or 90 per cent. of the cases of obstruction of the pelvis forbidding the delivery of the fœtus in the natural way, might be saved by a timely resort to Cæsarean section.

There is a serious moral aspect of this matter which we are in duty bound to consider. It is our object to save life, not to destroy it. Besides this, the innocent fœtus has rights which it should be our care to protect.

DISCUSSION.

Dr. W. D. Haggard, of Nashville, Tenn.—I rise to emphasize the words communicated by my friend, Dr. Kollock, in his valuable paper.

The ground upon which he bases the opinion that craniotomy

is not a justifiable operation is a legitimate deduction from the arguments adduced.

At the Nashville meeting of this Association, in 1889, I read a paper entitled "The Improved Cæsarean Section Versus Craniotomy." (See TRANSACTIONS of the Association for 1889, p. 66). A somewhat extensive reading and study of the literature of the subject since that time has confirmed the opinions therein detailed. In that paper I took substantially the same ground occupied by Dr. Kollock, and I feel proud to have him confirm the views I then held by illustrating in his own experience the correctness of the sentiments enunciated at that time.

It seems to me that when the profession at large come to realize the immense difference in the number of lives saved by Cæsarean section over that of craniotomy there can be no doubt as to the propriety of adopting the former operation in preference to the latter. In this connection I wish to still further emphasize the point made by Dr. Kollock in his paper, that we should operate early, before the patient has been exhausted by frequent attempts at delivery by the forceps; before the woman has been exhausted by long delay and anxious forebodings.

There is a growing necessity for the profession at large to study the technique of craniotomy more thoroughly, that they may be enabled to realize the dangers to which the woman is exposed, and the fearful responsibility they assume in the destruction of a living child. It is folly to attempt to deliver a woman when the pelvic obstruction is such as to render the delivery impossible by the use of the forceps. Then why not determine this fact in advance, and resort to the Cæsarean section before subjecting the woman to the dangers incident to delay and fruitless attempts with forceps, which only serve to produce contusions and lacerations of the soft parts of the mother, thus inciting septic infection, which is often responsible for the woman's death—credited to the Cæsarean section itself.

DR. HUNTER MCGUIRE, of Richmond, Va.—I only rise to say that Dr. Kollock's paper is most timely, and I wish it could be put in the hands of every practitioner in this country.

Some time ago I saw the report of a case by Dr. Thomas, of New York, where, in doing a Cæsarean section, he proposed to take the uterus out of the abdominal cavity and then to open it.

Coming from such an eminent authority, I thought it calculated to mislead. To my mind it would add very much to the danger of the operation; it would make it necessary to open the abdomen to an unusual extent to get the uterus out, and expose the cavity of the abdomen a long time to the atmosphere; therefore, I simply wish to state that I think it is unnecessary to take the uterus out of the abdomen. In one case I did about a year ago the assistant kept the uterus close up to the abdominal wall during the whole of the operation. I do not think any blood entered the cavity of the abdomen. An incision was made and the child removed, the uterus being all the while in the abdominal cavity. I simply mention another fact: that the uterus was slow to contract, the woman was very feeble, and I used electricity (faradic current) to make the womb contract. It accomplished this very rapidly. One pole was placed directly over the uterus. In this case both mother and child did well.

Dr. L. S. McMurtry, of Louisville, Ky.—I simply want to call attention to one point in connection with this very important subject which Dr. Kollock has brought before the Association. It is a subject in which very brilliant work has been done in the last few years, and professional opinion is very rapidly changing in regard to it. A few years ago it would have been impossible to have presented the views that Dr. Kollock has presented this morning and to have had them accepted without very violent opposition, because Cæsarean section has been regarded as an extremely heroic operation, and until recent years the mortality from it has been very great. But since the improved Cæsarean section has been carried to the present degree of perfection, it has strengthened the opinions of abdominal surgeons and has increased the favor in which the operation is presented. Within the last two months symphysiotomy has been brought before the profession and practised in this country as an alternative in certain cases where the Cæsarean operation would be considered. What the future of that operation is to be we are not prepared to say. But, Mr. President, in those cases where craniotomy or a Cæsarean section must be performed we have got to elect between several courses, and there is one course which is more easily carried out than a Cæsarean section, which is much safer than a Cæsarean section, and which, when a skilled abdominal surgeon

cannot be had, is more apt to succeed in the hands of a surgeon or obstetrician of limited experience. I allude to the Porro operation—an operation which consists in opening the uterus and then removing the organ, conjoining a hysterectomy to a Cæsarean section operation. At first sight it would seem to be a much more serious operative procedure than a Cæsarean section, but just look at it: A succulent uterus with several rows of stitches, and containing inside of it the débris of labor, is returned to the abdominal cavity. We have a very grave danger there to which the patient is subjected. But in all those women in whom a Cæsarean section is necessary it is a good thing to put them in a condition that they cannot become impregnated again. In many instances of Cæsarean section an operation has been performed as often as two or three times upon the same subject. You return the uterus, and it all heals, the woman is in the same dilemma and emergency that she was for the primary operation. After you open the abdomen and open the uterus, deliver the child, and hand it to an assistant, you have a uterus that has no attachments or adhesions. I refer now to the operation of supra-vaginal hysterectomy rendered ideal to an extent. You turn the uterus out, place at the junction of the cervix with the body of the uterus the wire of Kœberle's nœud, amputate the uterus, fix the stump in the lower angle of the wound, and you place the woman in a condition to get well. It is far in advance of a Cæsarean section. It is an operation that is far less dangerous, and in the hands of one American surgeon it has been performed four times with a saving of all the women and loss of only one child. I do not think any operation of that magnitude can show such a result. It is an operation which Mr. Tait claims is one of the simplest in abdominal surgery, which only needs six instruments for it to be done with.

Dr. Arch. Dixon, of Henderson, Ky.—I desire to relate a case in order to emphasize the fact brought out by Dr. Kollock, and also to add my indorsement to that of Dr. McGuire in regard to Dr. Kollock's paper, namely, that the paper should be put in the hands of every practitioner in this country. About six months ago I was called in consultation to see a woman who had been in labor eight or ten hours. The conjugate diameter was less than two inches. After a thorough examination I proposed to do a Cæsarean section. The physician in attendance was like

the physician described by Dr. Kollock—he had an objection to even looking at a knife, much less having to use one. He objected and insisted on my doing a craniotomy. The family also objected, and craniotomy was done, and the child delivered in a dismembered condition. While every precaution was taken to keep everything aseptic, the uterus washed out after delivery, iodoform tampon inserted, and everything that I knew how to do was done in order to prevent sepsis, the case was left in the hands of the practitioner, and in four days the woman died of pelvic peritonitis. I believe a Porro operation in that case would have saved the woman's life and perhaps the life of the child.

Dr. Kollock.—While the gentlemen have indorsed the views I advanced in my paper, I cannot agree with Dr. McMurtry about the danger of leaving the uterus behind *with its débris.* There is no necessity for leaving any débris in the uterine cavity. I washed it out thoroughly through the vagina, leaving nothing behind to cause septic infection. Speaking of Porro's operation and the possibility of infection, I will say I did Batty's operation and took out the ovaries, knowing that it would not increase the danger of the operation.

I had another case which I did not mention in my paper. It is a very interesting one. She was a young woman, lived some distance from me, was taken sick, and called in the family physician. Her mother suspected that she was pregnant. He examined her, and said she was. "She cannot be delivered," he said, "on account of a contracted pelvis," and wrote asking if I would operate on her. I said I would. He sent her in time, so that she could be rested before labor came on. She did not expect to be confined until the middle of September. She arrived at my office on the 11th of August, and labor came on on the 14th, so I had to operate at once; no time to chat. I got the child out, it weighed 8½ pounds, and is living to-day. The woman was paralyzed from her hips down; has not good use of her hands. She cannot use a knife and fork at the table till her food has been cut up; after this she can feed herself.

I was forty-seven minutes in completing the operation on her. She went on doing very well till the third day, when the temperature was 100°. I washed out the cavity, gave her an enema, and it was reduced to 98° without any trouble. I am satisfied

if the operation is done in time the mortality would not be anything like that of craniotomy, as I believe they would nearly all survive. I believe patients will stand the operation of Cæsarean section as well as an operation for the removal of an ovarian cystoma. The latter is in a state pathological, while the pregnant uterus is in a healthy physiological state. I would not hesitate to operate if I got my patient in time. Those cases operated upon early nearly all recover. Zweiffel saved 29 cases out of 30; Schroeder 14 out of 16; Schauta 15 out of 15; H. A. Kelly 4 out of 4; Joseph Price 7 out of 8; Charles Jewett 2 out of 2; and H. C. Coe 2 out of 2.

A CASE OF EXTENSIVE HÆMATOCELE RESULTING FROM TUBAL PREGNANCY RUPTURING INTO THE BROAD LIGAMENT.

By W. D. Haggard, M.D.,
Nashville, Tennessee.

Miss K. C., house-girl, aged twenty-four years, jumped from an express wagon July 4th. Her menses were past due. Shortly afterward she was seized with pain in lower abdominal region, which grew worse during the night. Next morning a slight flow from the vagina was apparent, which she regarded as a return of her menses. For several days the discharge increased, and the pain gradually grew worse. One week after the accident the family physician was called. He proposed vaginal examination, but was refused. Opium and ergot administered without effect.

One week later, another physician was called, who likewise asked for a vaginal examination, but without avail. At the expiration of the third week there was no improvement, and the patient was taken to the Catholic Hospital. Dr. Coyle, the physician-in-charge, found an ovoidal tumor, as large as a cocoanut, above the symphysis pubis. It was very tender, non-fluctuating, and occupying almost a central position. As a vaginal examination was denied, he supposed the tumefaction to be a gravid uterus. The patient became dissatisfied at the hospital, and returned home. On the twenty-ninth day I was called to her, to meet Dr. Coyle in consultation, to whom I am indebted for the foregoing history.

Examination. Patient was found anxious, restless, and impatient, suffering greatly from pain in lower abdominal and pelvic region. Temperature 103°; pulse 120; skin hot and dry;

tongue heavily coated; intense headache; globular tumor above pelvic brim, so tender as not to permit of palpation. Bowels tympanitic and tender throughout. Vaginal examination revealed left lateral pelvic space completely occupied by a non-fluctuating tumor, so exquisitely tender as to preclude a thorough examination.

The tumor in the hypogastric region was supposed to be an extension of the pelvic enlargements. The womb was closely hugging the symphysis; the external os patulous, discharging considerable blood, and on a level with the arch of the pubes.

Her condition was most critical; her position as a house-girl anything but consoling. The possible rupture of the encapsulating peritoneal membrane into the general peritoneal cavity with escape of contents, and the inevitable peritonitis, was feared. Vaginal puncture and drainage was discussed and discarded. Section urged as the only hope. Dr. Duncan Eve, who was added to the consultation, concurred. The patient, after a day's hesitation, consented to an operation, and was removed to St. Margaret's Hospital.

Operation—12 M., Sunday, August 7, 1892. She took ether badly. Median incision. Anterior wall of tumor was adherent to parietal peritoneum, hence they were incised together. The operation was practically extra-peritoneal. About three pints of dark clotted blood turned out. Care was taken not to break down the partition which so effectually walled off the hæmatic tumor from the abdominal viscera. The sac was quickly and thoroughly irrigated, sponged dry, and operation hurriedly completed. She rallied badly. The intense pain and restlessness, which had been partially controlled by morphine for the last thirty-six hours, became uncontrollable. She died at 12 P.M., twelve hours after operation.

Post-mortem, two hours after death, under great disadvantages. Incision reopened, adhesions between sac and abdominal wall broken. The entire abdominal viscera of deep modena color, matted together in places from peritoneal inflammation. No blood found in abdominal cavity. Pelvis carefully examined; viscera fused together in a conglomerate mass. Uterus sought for and found enlarged; tubes not recognizable. The pelvic viscera removed *en masse*. The rectum adherent and perfectly

black. Uterus measured antero-posteriorly, $1\frac{1}{2}$ inches; transversely, $2\frac{5}{8}$ inches; longitudinally, $5\frac{1}{2}$ inches.

Microscopist's report. "The specimen (endometrium) given me for examination was submitted to the usual preliminary procedures, and found to consist of large embryonal cells completely filling the spaces of a myxomatous reticulum. The cells were clearly the so-called 'decidual cells,' having a round or oval shape, and containing one or more nuclei; they varied in size, having a general average of about one-four-hundredths of an inch in diameter, or eight times the diameter of a red blood-corpuscle. Remnants of utricular glands were present, but not numerous. Groups of connective-tissue corpuscles were scattered here and there. The specimen showed clearly that decidual formation had taken place.

"HUGH R. MILLER, M.D."

Although the fœtus was not found, that it was a case of tubal pregnancy with rupture into the broad ligament is clearly established by the clinical history and post-mortem appearances, which may be summarized as follows:

1. She confessed having had intra-pelvic trouble previously (presumably gonorrhœa), for which she was treated locally.

2. At the time of the accident, caused by jumping from a wagon, her menses were past due; as to how long, her statements were misleading.

3. There was a fitful yet persistent bloody flow from the uterus during her entire illness.

4. Paroxysmal, colicky pains in the lower abdominal and pelvic regions were of frequent occurrence.

5. Existence of a tumor above the pubes, which she probably mistook for a gravid uterus.

6. Persistent refusal to submit to a digital examination, probably fearing the detection of her pregnant state.

Post-mortem appearances. 1. Enlarged and softened condition of the uterus with a patulous os, showing the escape of a sero-sanguinolent, stringy fluid.

2. Enlargement of the left tube with a well-defined cavity, from which the fruit-sac escaped.

3. Existence of a deciduous membrane, as revealed by the microscope.

4. Discoloration of the rectum, produced by blood dissecting around it, producing constriction and partial death.

DISCUSSION.

Dr. Henry O. Marcy, of Boston.—Mr. President, before speaking on this subject, I take pleasure in expressing my thanks to Dr. Haggard for his admirable report of one of those difficult, doubtful, and troublesome cases that tax the best skill of the surgeon, and that cause him to feel utterly provoked that he could not have had his own way and operated at the time he made clear his diagnosis. This woman was offered the best possible advantages, but declined them.

The point I wish particularly to call your attention to is the condition of the endometrium. Some of you doubtless remember that, not many years ago, I published the exhaustive studies of Professor Ercolani, translated from the Italian, upon the productive process of the mammalia, including woman. Years before, I had met accidentally a report of some of his earlier studies which had received the prize of the French Academy, and which had been published in Algiers, Africa. It contained what seemed to me a clue for the unravelling of the complex and hitherto little understood developmental processes of fœtal nutrition. I instituted a series of careful investigations which demonstrated to me the truth of the remarkable studies of this great anatomist, and, as a labor of love, gave quite a year in the preparation of the work for the benefit of the English-speaking members of the medical profession.

The demonstration is ample that the primal plan of placental nutrition in all the vertebrates, reduced to a simple expression, is a villus of secretion (glandular, maternal) and a villus of absorption (fœtal). This, in the simpler forms of placental development, as in the mare, continues to the end of gestation as a double layer of interdigitating villi, the villus of secretion and

that of absorption being placed in close juxtaposition, each being covered with its independent layer of epithelium. In the cow, the placental development is of a much more complex form, represented by many cotyledons (the multiple placenta of ruminants). The unity of type in placental development, however, is nowhere departed from in the more complex development of the placenta in woman.

In the very earliest stage of fœtal development we find that the nutrition of the embryo is carried on by the interdigitating of the villi—the so-called shaggy coat of the chorion—with the maternal villi, the decidua vera. At about the third month the uterine mucosa and fœtal villi undergo atrophic changes, except at the serotinal site, and here develops the complex and hitherto little understood organ called the placenta. The fœtal villi, when simple, are only in contact with the decidual cells. Little by little they proliferate, becoming arborescent. The decidual membrane which covers them is thus variously intra-folded and becomes ectasic. This protrusion necessarily takes place by the introflexion of the walls of the vascular structures which are pressed upon by the proliferating chorial villi, and the cells of the decidua are maintained unchanged, even when the placenta has completed its development. Thus, instead of the general belief that the nutrition of the fœtus takes place through an osmotic exchange of the two bloods, it is clearly demonstrated that there is a single law in physiological modality, that the placenta in all animals is glandular, and that the nutritive material which is to serve for the growth of the fœtus in all the vertebrates is furnished by the mother. In mammals, it is supplied by the maternal portion of the placenta gradually, to correspond to the need of the developing fœtus.

In extra-uterine fœtation, as in the case reported, we have a beautiful demonstration of a law of physiological modality in the development of the placental cells, which, as a thick coat, line the interior of the uterus. It is the decidua vera of pregnancy, and although the uterus itself may never have contained the fœtus, when demonstrated, impregnation cannot be doubted.

Dr. Joseph Taber Johnson.—I would like to ask Dr. Marcy if he can give us any special information about those cases where the placenta is said to grow after rupture of a tubal preg-

nancy. The placenta attached to some abdominal viscus still continues to grow after the death of the fœtus. Mr. Tait, I believe, says it grows in quite a number of cases. It bothers a great many practitioners to see how it grows.

Dr. Marcy.—In reply to the question of my friend Dr. Johnson, I would say that the studies of Professor Ercolani go very far to explain the limitations of ectopic pregnancy. They confirm the teachings of Mr. Tait, that an ovarian pregnancy is very likely impossible, since the ovum after impregnation must have a more or less suitable soil for its implantation, and this cannot be furnished by the ovary or peritoneum. The mucous membrane lining the Fallopian tube possesses the fundamental factors for placental development. The fœtal villi are here covered by a layer of cells furnished by the endothelial walls of the enlarging and newly formed capillaries—the placental secreting cells. Thus the placenta has its development as within the uterus, and when the tube has ruptured and the placenta continues to grow, as in rare instances, the vascular supply for its further nutrition may be derived from its attachment to surrounding organs.

Dr. James T. Jelks, of Hot Springs, Ark.—I differ with Dr. Haggard in his diagnosis of this case as one of extra-uterine pregnancy. It was unquestionably a rupture, but I do not believe, from his description of the case, it was a rupture into the broad ligament. The sac he found was an inflammatory one, thrown out by Nature to wall off the ruptured material. It has evidently been some weeks since rupture took place. We know as a matter of fact that rupture into the broad ligament is a rare occurrence. It was my pleasure to see Dr. Price do a laparotomy for extra-uterine pregnancy, and in speaking of it, I do so to condemn the use of electricity in such cases. He drew out a fœtus about twelve inches long. The woman had received electrical treatment for three months in the hope that it would produce absorption of the fœtus which existed in the abdominal cavity. The fœtus was floating free in the abdominal cavity with a pint of decomposing fluid, yet the physicians had been using daily applications of the galvanic current to produce absorption of the exuded fœtus, showing the absurdity of some physicians who attempt to make electrical applications in cases of extra-uterine pregnancy. It is true that in the first six weeks we may

destroy the fœtus with the galvanic current. Nature may absorb the child and the patient thus escape. After that it is a mistake to use electricity in these cases.

We cannot easily make a diagnosis prior to rupture, but after rupture diagnosis is easy. It is folly to attempt to use electricity for absorption of the fœtus after rupture. The only remedy is the knife, which should be resorted to promptly and quickly.

Dr. G. Frank Lydston, of Chicago.—I hardly feel myself competent to discuss the interesting subject of extra-uterine pregnancy, but Dr. Jelks's remarks have suggested to me the fact that abdominal surgeons are not sufficiently emphatic and general in their condemnation of the methods of the electrician, probably because they do not insist with sufficient emphasis upon the well-nigh impossibility of making a correct diagnosis of extra-uterine pregnancy prior to rupture, or prior, in certain exceptional cases, to the time when the fœtus is well developed, and when any idea of using galvanism should be entirely out of the question. This matter was brought to my mind not only by the remarks of Dr. Jelks to the effect that electricity was perhaps justifiable or effectual up to the sixth week, but by a case that I heard described a short time since in the Chicago Medical Society by one of our prominent electro-maniacs. The case was diagnosed as one of extra-uterine pregnancy in the sixth week. He could feel the little fœtus, according to his account, to use an expression of Dr. Price, "roosting" up in the pelvis. As soon as he applied the electric current the symptoms disappeared, and the case was "cured."

I think that one point should be strongly emphasized (a point which is especially brought out by such cases as have been reported), namely, that it is absolutely impossible to make a diagnosis in these cases early; a great many practitioners will then stop experimenting with the electric current. Until rupture or inflammatory complications have occurred, there is very little glory for the surgeon and very little safety for the patient. If we condemn the idea of the possibility of making an early diagnosis in these cases, I think we will perhaps go a little farther in the condemnation of the methods of the electrician. So long as the tyro flatters himself that he can make an early diagnosis of extra-uterine pregnancy, so long will we hear of wonderful

cures by electricity. The time has arrived, I think, for the expert to sit down good and hard upon the bumptious pretensions of such men as the one to whom I have alluded as claiming to have made a positive diagnosis in the sixth week.

Dr. W. E. B. Davis, of Birmingham, Ala.—I want to indorse what has been said in regard to the infrequency of rupture into the broad ligament. We have an analogous condition to this in appendicitis, in the inflammatory process that precedes rupture. When we have a rupture in ectopic gestation there is an inflammatory process, and the blood is thus walled off. The so-called ruptures into the broad ligament are usually ruptures into the general cavity which have been protected by adhesions.

So far as the treatment by electricity is concerned, it is hardly pertinent to the paper. I do not think there is any question but that some cases can be relieved with it. It is bad practice, however, to use electricity in these cases, since abdominal section is decidedly more reliable. Furthermore, after a section nothing is left to give trouble subsequently.

Dr. Haggard.—I am thankful to the gentlemen who have discussed my short paper, and especially to Dr. Marcy for his scientific analysis of the formation of the deciduous membrane.

With reference to the remarks of Dr. Jelks, I beg leave to say that the anatomical classification of pelvic hæmatocele into intra- and extra-peritoneal renders the differential diagnosis between rupture of an ectopic gestation into the abdominal cavity and rupture between the folds of the broad ligament much less difficult, but does not render the diagnosis of either possible, except perhaps in very exceptional cases, and then only in the hands of an expert diagnostician, before rupture occurs.

In the case I had the honor to report, it is my opinion that if the rupture had been intra- instead of extra-peritoneal, the woman would certainly have perished from the extreme loss of blood—since in the intra-peritoneal variety there is nothing to restrain the loss of blood, while in the sub-peritoneal the flow is restrained by the peritoneal membrane behind which the blood is effused. This was plainly shown by the semi-gangrenous condition of the rectum, which resulted from the effused blood pushing its way by pressure between the peritoneal and muscular coats of the gut, thus producing a constriction whereby the

circulation was interfered with. A moment's reflection will convince anyone that this pathological condition of the gut could occur in no other way than by pressure from a sub-peritoneal effusion. If the gentleman wants still further proof of the fact that it was a case of ectopic gestation, it may be found in the report of the microscopist, who found deciduous formation in the lining membrane of the uterus. The results of inflammatory action, as displayed at the post-mortem, by which Dr. Davis seeks to account for the pathological condition found, was the result of pressure and interference with the circulation by the extensive effusion.

In closing my remarks, I would like to emphasize the necessity for further study by careful, painstaking autopsies and reports of all cases resulting in death. This would result, as I believe, in establishing the fact that by far the greater number of pelvic hæmatoceles result from tubal pregnancy; besides, this study would give us a clearer conception of the relative frequency of intra- and extra-peritoneal ruptures.

FIBROID TUMOR OF UTERUS; PREGNANCY; RUPTURE ABOUT THE FOURTH MONTH; OPERATION; POST-MORTEM SPECIMEN.

By S. M. Hogan, M.D.,
Union Springs, Ala.

M. W., aged twenty-eight years, black, was examined by me May 9, 1892. Last child seven years old. Married last two and a half years. Last sickness December 12, 1891, but since then has had a show twice, but slight. About six weeks ago had very severe pain in the left side, followed by a serious hemorrhage from womb, and a fainting spell.

Soon afterward she was seen by Drs. Walker and Ziegler. They reported fever very high, but temperature was not given. Bowels constipated; pain and tympanites very great.

She was afterward seen by Dr. Thomason who treated her with cathartics and opium. Retention of urine was an early and constant symptom, necessitating the frequent use of the catheter. His diagnosis was peritonitis, a tumor, and probably malignant. He sent her to me for an operation May 9, 1892.

She was brought in a wagon, and arrived at my office at night, after travelling about fifteen miles.

As there was such a pressing desire to urinate, with only a few drops passing, a catheter was introduced and about eight ounces of very strong ammoniacal urine drawn off.

Abdomen was very much enlarged, and so tender as to prevent a thorough examination.

On vaginal examination a fœtus could be easily felt through what seemed to be an exceedingly thin membrane. The shoulder and head presented, but it was absolutely impossible to find the os. The feet could be felt, as though just under the skin, and to the left of the umbilicus.

Pulse 150, temp. 102.5°; bowels constipated and extremities cool.

Drs. Hayes, Hunter, Franklin, Cowan, and Bledson were invited to see the patient; but, owing to her extreme condition and the intense pain she was suffering, a thorough examination could not be made without an anæsthetic. We decided that it would not be advisable to give it then.

She was given hypodermically, morphine, grain $\frac{1}{4}$; atropine, grain $\frac{1}{100}$; then a tablespoonful of Epsom salts was ordered to be given every three hours until free catharsis was obtained.

May 10th, 9 A.M. The salts acted well; pulse 130, temp. 101.5°; pain less. Having taken some nourishment, she felt more comfortable. Eight ounces of urine were drawn off. By placing her on left side, I succeeded in finding the os, to the right and above the pubic bone, and near its junction with the ileum, or rather in the extreme right lower lateral portion of the iliac region. It seemed to be very much entangled, but, owing to its peculiar position, I was unable to introduce my finger into it. The child was lower in the pelvis, but the pains were more bearable.

May 11th. Pulse 140, temp. 102°; suffering more pain. Another saline cathartic was given; although it acted well, the pulse increased in frequency to 150; temp. 103.5.° On the morning of the 12th, she was in great pain, which was intermittent in character and very similar to regular labor pains. The child had descended almost to the vulva.

Every effort had been made, without using an anæsthetic, to form a correct diagnosis, some holding the opinion that it was a case of ectopic pregnancy; and others, that it was a dilatation of the posterior segment of the uterus having exceedingly thin walls. This was my opinion. It was also my opinion that a Porro operation was the thing to do.

On the 10th and 11th, she had a thorough bathing with hot water and soap. On the morning of the 12th, in addition to this, her abdomen was thoroughly bathed with a solution of bichloride mercury, 1 : 1000, and enveloped in hot towels wrung out of a weaker solution, 1 : 5000. With the assistance of Drs. Hunter, Hayes, Franklin, and Bledson I proceeded to operate. An anæsthetic of ether and a small quantity of chloroform was used. The towels

were removed from the line of incision and abdomen bathed with alcohol. An incision was made from near the umbilicus to near the pubes and the uterus exposed, but the opening was not sufficiently large for it to be lifted out of the abdomen. Rather than make the incision longer, I decided to open it in the position in which it then was. On the first stroke of the knife I found I was cutting into a solid tumor. I then passed my hand behind and to the left, and found the feet of the fœtus, with the legs protruding from a rent in the left side of the uterus. It was easily removed. After breaking up some considerable adhesions, the uterus was lifted from the abdomen, an elastic ligature applied. Two long transfixion pins were introduced and the uterus removed; the abdomen was washed out with hot water and a drainage-tube introduced; the wound was closed with silk thread, dusted with iodoform; iodoform gauze applied with absorbent cotton and the usual bandage.

The patient was then put to bed and hot applications applied to the extremities and a hypodermic of atropine and digitalis given. This was about 12 M. At 3 P.M. I saw her again; pulse about the same, temperature not taken. She was not only comfortable but cheerful. No nausea from the anæsthetic, but she was very thirsty. One teaspoonful of water was given every five or ten minutes, with very great comfort. The nurse said: "About 9 P.M. I left the room, but only for a few moments. When I returned I found that the patient had raised herself up, and had attempted to get a glass of water which had been left near the bed; but she suddenly dropped back, and when I got to her she made one gasp and was dead."

It might be claimed that with better surroundings and better nursing she might have recovered.

I do not think so, for there was some indescribable something about this patient, even the first day I saw her, that made me feel sure of the issue, and I expressed the opinion that she would die.

While I felt sure of the result, still I believe the operation was imperatively demanded, and I would again operate if the same symptoms were presented.

Now, here was a case of fibroid tumor of the uterus, and

from the symptoms and history of the case there was surely a rupture, and the probabilities are that it was about the fourth month of gestation. I am also of the opinion that the rupture did not immediately destroy the fœtus—that it continued to grow in its abnormal position. This specimen that I show you, I think, will convince you that it is about a five months' fœtus. The patient also stated that she felt the movement of the child quite a number of times after she had the fainting spell.

You will also observe in this specimen that the placenta is attached to the thinned sacciform portion of the uterus, and in places it is tightly adherent, the result probably of the inflammation following the rupture. The tumor is very much smaller than when first removed.

Although the adhesions were considerable they were easily detached, and with very little hemorrhage or oozing, which was easily and completely controlled by hot-water irrigation.

Early next morning, Dr. Hunter opened the abdomen and removed the cervix, broad ligaments, tubes, and ovaries, which I here show you. You will observe at the fimbriated extremity of the left tube there is a small hæmatoma, and the right ovary quite small. He reports that the bladder was very much inflamed, thickened, and elongated; that the kidneys were much larger than natural, and that there were evidences of general peritonitis. There had been no adhesions to the bowels at any point; but the adhesions in the pelvis and the excessive amount of thickened tissue clearly indicated that the inflammation had continued for some considerable time. There had been no hemorrhage.

From the history of this case and from what we know of the mistakes that have been made in cases of appendicitis and pyosalpynx, would it not be reasonable to conclude that others have died from the same cause, without their true condition being recognized? Then, if this be true, how important it is that we make an early and correct diagnosis; so that the best treatment could be instituted to give the greatest amount of recoveries.

Now in regard to the diagnosis. If a rupture had taken place before the patient was seen, with the symptoms present as in this case, we could easily settle the question of pregnancy by vaginal examination, but it would be very difficult, if not impossible, to decide that there was a tumor, unless we could get a history of a tumor being present prior to the pregnancy; still, enough symptoms would be present to justify an operation. If, however, there had been no rupture and no inflammatory symptoms present, then by bimanual examination a tumor ought to be found. If the pregnancy had advanced to the third or fourth month, and had been attended with the usual symptoms of pregnancy, we ought to be able to arrive at a correct conclusion. We ought also to be able to determine, from the size of the tumor, the position of os, and the condition of the uterine walls, what course to pursue. But if the pregnancy had only advanced to the first or second month, the difficulties of making a diagnosis would be increased, but the danger of delay would be less.

In looking over the reports of such cases, we find some have gone to the full time, and have been delivered with safety to both mother and child, but more frequently abortion takes place.

Dr. Joseph Price reports three cases, two operated upon near the full time, and one at six months. Dr. M. Price operated upon one case at three months. All of these cases recovered. It is very evident that the operations were not only justifiable but absolutely necessary. Surely no one would doubt the propriety or necessity of operating after rupture had taken place; I feel sure if it had been done immediately in this case, the results would have been very different—the patient could have been saved.

In conclusion I would say: In all cases of rupture, the Porro operation should be performed immediately.

In all cases where the tumor is large or multiple, intramural or subperitoneal, with a sacciform dilatation of the posterior segment of the uterus, and the os above the pubic bone, or inaccessible, the same operation should be done.

In all cases where the tumor is in front of the child, or blocking the passage, it should be done, provided the pregnancy has advanced to the full time, or there should be a hemorrhage, or rupture of the membranes, indicating that an abortion or miscarriage is imminent.

A laparotomy should not be done simply because a woman is pregnant and has a tumor. Let us always have other and more positive indications for performing an immediate operation.

DISCUSSION.

Dr. James T. Jelks, of Hot Springs (opening the discussion).—I would like to discuss Dr. Hogan's paper very briefly, and desire first to ask him a question. How long has the pregnancy existed?

Dr. Hogan.—About the fifth month. About the fourth month rupture took place.

Dr. Jelks.—Dr. Hogan is one of my medical fathers, and I do not like to differ from him, but I am obliged to do so in this case. In the specimen before us we see the tube which was taken out post-mortem, also the broad ligament, the cervix, and a rupture in the Fallopian tube which you can see, and which the Doctor considers a hæmatoma. With all deference to the Doctor's opinion, I should say that he had to deal with a ruptured tubal pregnancy. He cut into the uterus and found what we would naturally expect to find—extra-uterine fœtation at four months, with an enlarged uterus, not any larger than it ought to be, and the uterine walls not thicker than they ought to be. Of course, we make some allowance for the specimen having shrunk. I beg to differ from the position he takes that it is a ruptured uterine pregnancy with the child in the abdominal cavity. I do not think it is. We have here the fimbriated extremity of the tube that is ruptured, with the child floating in the abdominal cavity. The mother lived for one month after the rupture took place. I should say it was a ruptured tubal pregnancy. It seems that it is the rational explanation to take, and that it would explain the matter a great deal better than to take the position of Dr. Hogan.

The President.—Have you any fibroid tumor complicating the pregnancy?

Dr. Jelks.—I do not think there is. This matter of extra-uterine fœtation is an exceedingly common affair, unless Dr. Price gets us his cases to order when we visit him. He has reported eighty-one cases. I saw him operate on two. One child, a good deal longer than this, was floating free in the abdominal cavity among the intestines. There was a ruptured tube such as we have in this case. My diagnosis would be a ruptured tubal, instead of a ruptured uterine, pregnancy.

Dr. J. McFadden Gaston, of Atlanta.—I had the opportunity of seeing the specimen in which Dr. Mordecai Price removed a large fibroid tumor, which corresponded, in many respects, to Dr. Hogan's, although it was much larger. The womb was still retaining the child on one side of the fibroid tumor. It was only detached after the successful removal by a Porro operation. Judging from the remarks of those who have spoken, I should say the case of Dr. Hogan was similar to Dr. Price's. Dr. Hogan misled us a little by using the term "down in the vagina."

Dr. Hogan.—I meant low down in the pelvis and pressing toward the vulva.

Dr. Joseph Taber Johnson, of Washington, D. C.—I feel satisfied, after listening to the paper and examining the specimen, that Dr. Hogan has had to deal with a ruptured tubal pregnancy. The woman lived six weeks after the rupture occurred. These diagnoses are very difficult to make, and it is one of those cases where the practitioner's eyesight is better than his foresight. We come to conclusions unaided by personal contact with the case which Dr. Hogan had. My diagnosis would be ruptured tubal pregnancy with coincident enlargement of the uterus.

Dr. W. E. B. Davis, of Birmingham, Ala. Dr. Hogan's case illustrates one very important point, namely, how difficult it is to accurately diagnose such cases before operation, and how we are sometimes unable to make a diagnosis afterward. It teaches the important lesson that it is not an easy matter to diagnose these cases with the specimen in hand. I have been impressed in looking at the specimen that the case was one of ectopic gestation with rupture.

Dr. Hogan.—Dr. Jelks, in his criticisms, seems to have lost sight of the fact that the opening at the fimbriated extremity of the left Fallopian tube was a post-mortem opening and not a pathological rupture, which I stated in showing the specimen. Another point I would again emphasize, was the position of the fœtus, the head presenting at the vulva, and also the position of the os, which was in the lower portion of the right iliac region.

In extra-uterine pregnancy with rupture, and especially at the fourth month, the hemorrhage is considerable, resulting in a large hæmatocele, which was not the case in this one.

The greatest difficulty we had in perfecting our diagnosis was in excluding a tubo-uterine pregnancy, but there was no enlargement of the tube at the tubo-uterine junction, and when we lifted the uterus from the abdominal cavity it presented an extremely natural appearance, and there was no difficulty whatever in using the elastic ligature, transfixion pins, and removing it. The placenta was inside of the uterus, and the cord was protruding from the rupture, which had been considerably enlarged in removing the fœtus.

It was thoroughly examined after removal by all present, and my diagnosis confirmed ; every point was clear and unmistakable, and I have no hesitation in saying that the pathology of extra-uterine pregnancy will not have to be re-written—at least, for the present.

BAXTER'S OPERATION FOR THE RADICAL CURE OF INGUINAL HERNIA.

By G. A. Baxter, M.D.,
Chattanooga, Tenn.

I desire to present to the Society a new operation for the radical cure of inguinal hernia, and to enumerate a few of what I conceive to be its merits. In doing so, I have no desire to decry any of the open methods now in use, all of which, especially the McBurney operation, have received well-merited favor from the profession, but simply to add another procedure which, I trust, will prove useful in dealing with a large class of cases of this otherwise very troublesome affection.

The procedure consists, in brief, in a prolongation of the incision through the internal opening upward into a more or less extensive laparotomy, as the exigencies of the case demand; dissection of all or a portion of the sac carefully from all its attachments within the canal and at the inner ring; ligation of the sac at some point within the canal; lifting the sac upward into the line of the abdominal incision *above the ring, and fixing it there;* and subsequent closure of the abdominal incision, canal, and scrotal openings after the manner described herein by diagram and in detail.

The first part of the incision is made in the usual manner over the tumor. The sac is not now treated differently from usual, except that, being dissected out or cut off within the canal after being ligatured, it is *not* dropped back into the cavity of the abdomen, but carefully retained outside the abdominal wall. The sac is now carefully dissected from around the internal ring and slightly separated from its internal

border, the ring being freshened and made as free as possible, after which the incision is carried directly upward through the inner opening sufficiently far to enable the sac to be engaged in it without being too much folded upon itself. The advantage of the upward incision is that, whether the hernia be an ordinary oblique inguinal or a direct inguinal, the deep epigastric artery is avoided. The object in making the incision in the abdominal walls sufficiently long is to prevent a folding of the sac on itself and thus get a closer and firmer union within the line of the incision with the parietal peritoneum, and a more extensive attachment to the abdominal wall. (Fig. 1.)

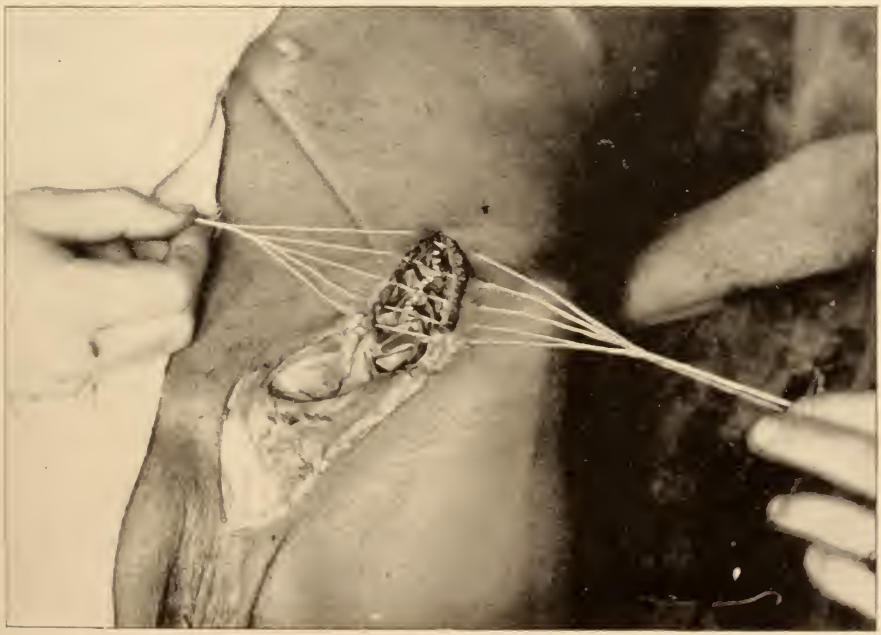
The next step consists in fixing the sac in the line of the abdominal incision. This is done by deep suturing carried downward through all the tissues, including the peritoneum on one side, thence through both layers of the sac and reversed on the opposite side from the abdominal cavity to integument, which sutures are not yet closed, but cut off long and laid back on the surface of the abdomen until all the sutures in the abdominal incision are passed. An all-important fact must here not be overlooked, the tendency of the fascia of the abdominal wall, owing to the peculiar and divergent directions of their fibres (and indeed the same may be said of the muscular tissues as well in this situation, for the incision is outside the rectus muscle) to contract underneath the integument, and the possible failure to properly coapt the different elements of the wall, tissue to tissue, owing to this contraction, without greater traction than is justified in the proper closure of the wound. Hence, an assistant should draw forward these contracting tissues flush with the integument on both sides, both during the passage of these first deep sutures and the subsequent more superficial layer. (Fig. 2.)

This deep layer of sutures passed, which are intended to approximate the peritoneum and hold the sac in place, the sac is now cut off in the depths of the wound, shortly above the peritoneum, and closed by a buried absorbent suture, and the second set of sutures is passed, an assistant counteracting, as

Fig. 1.

Sac dissected and raised from canal to internal opening.

Fig. 2.



Sac raised into abdominal incision, fixed with deep sutures, cut off and closed by absorbent suture.

before, the contraction of the tissues from both sides. These go only below the fascial tissues, passing across above the peritoneum and the now closed sac, and are meant to approximate these tissues, fascia to fascia, muscle to muscle, as these fascial tissues are regarded as the bulwarks against the possibility of ventral hernia; and it is desirable to bring homologous tissues into apposition, for only in this event can the resulting cicatrix be regarded as prominent. One other condition must here be mentioned. Unless this tendency of the fascial tissues to contract in different directions and withdraw underneath the skin is carefully watched by the operator and they are drawn flush by an assistant, who must see that every tissue is included in the grasp of his forceps during the passage of both sets of sutures, one or more coverings will fail of complete approximation, and though the wound unite and there be no resulting ventral hernia at the point of the incision, the general abdominal wall will be weakened in degree by the resisting force of this non-approximated fascia at one side or other of the incision ; and the expulsive force of the abdominal contractions being now taken off the ring entirely and deflected against the abdominal walls, there is likely to be a bulging of these walls, in time, at this weak point, and finally a resulting hernia. This protrusion, if it occur, will probably be on the outer side of the incision, as here the fascia, being longest, has more room for contraction. This danger, as that of ventral hernia in general, should be avoided by a careful attention, in detail, to the rules of closure laid down.

Before the closure of either of these two sets of sutures now passed, and after the cord has been placed to the outer side of the incision in the canal, two crucial sutures are passed at the internal opening of the canal from below upward, dipping over the cord but entering and passing through the tissues after the manner in the majority of operations for ruptured perineum—traversing them, in other words. These last sutures should now be closed first. But before being tied they should be drawn together to see that perfect approximation is

had at the internal opening, in order to permit the passage of a supplemental suture at this point in very large openings, while the approximation can be seen from below and above the opening (it can always be seen from below and felt from above) and, if found to be complete, they are tied; when drawn together the inner opening is found closed upon itself like the inner and middle contracting coats of an artery after ligature. The abdominal wound is now closed, commencing with the deep sutures and followed by the more superficial set—the fascial. (Fig. 3.) The hernial canal and the scrotal opening are closed with deep suturing. No drainage is used, unless the sac or its contents has been diseased, as complete obliteration of the whole canal by first intention is attempted, except the narrow canal through which the cord yet passes. One other precaution, perhaps, should be given for the benefit of those not accustomed to this class of operations—the necessity of a careful removal of all fat from the neighborhood of the internal opening and a freshening of the edges of the tissue at this point for better union. The points of originality claimed in this operation are: a line of incision through the abdominal walls suitable for any inguinal hernia; the attachment of the hernial sac in the line of the abdominal incision above the internal opening diverting the expulsive efforts from, and consequent pressure upon, the internal ring; and the method of closure of the abdominal wound so as to obviate the danger of ventral hernia—the latter point adapted to the exigencies of this operation from the laparotomists.

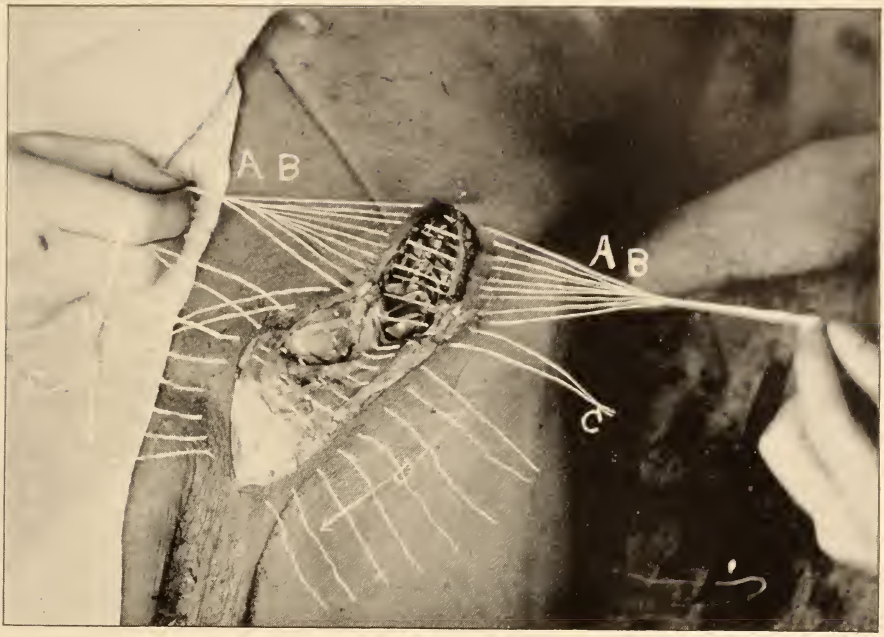
Its advantages, briefly enumerated, are considered to be:

1. It takes only the ordinary time for the closure of any wound to make firm closure of this, as there should be a union by first intention.

2. It obviates the use of a truss, as the pressure is no longer against the internal opening, and anything in the way of a truss more than a broad band in this situation would do harm by causing absorption of cicatricial tissue.

3. It deflects all expulsive efforts from the internal opening and canal to the abdominal parietes, thereby greatly lessen-

Fig. 3.



A. B. Deep and superficial sutures.
C. Crucial sutures at internal opening.
D. Sutures closing the hernial canal.

Fig. 4.

Result of Baxter's operation for hernia in Dr. A. W. Boyd's case.

ing the liability to a recurrence in the old channel; and by so doing it leaves the canal and ring in a perfectly quiescent state during the whole progress of healing.

4. It affords a smooth instead of a pouched covering to the internal ring, which covering has sometimes the advantage of being thickened.

5. It avoids any necessity for traction on sac or its contents, and hence lessens materially the danger of rupture of sac or contents in diseased conditions.

6. It allows complete exsection of sac below the internal ring, if advisable in diseased conditions, and affords abundance of room for a resection of gut if found necessary.

7. It is applicable to nearly all cases of inguinal hernia, is easy of execution, and affords an excellent result, with firm cicatrix and complete obliteration of the whole canal. Its two dangers are ventral hernia and pressure by contraction on the cord. The first does not seem to me so great, when care is taken to approximate homologous tissues, as it is through an imperfectly closed and obliterated ring and canal, or a perfectly closed one, with the changing conditions of its resultant connective-tissue cicatrix at that point and the constant pressure upon it. The danger from pressure, by contraction on the cord, is neither greater nor less than it is in all open operations which do not, as a part of their procedure, excise the cord and testicle.

I present to you, also, a photograph of a late result by this method, operated upon by Dr. A. W. Boyd, of Chattanooga, Tenn. (Fig. 4.) The patient was an old man with a large amount of adipose tissue. Hernia of the direct kind, the size of a child's head, and of many years' standing. The canal and openings so large as to make it an impossibility to retain it with a truss. The sac attachments were extensive, but so far as could be ascertained there was no history of strangury at any time.

The union here seemed complete, and the man "as well as ever," as he expressed it, in two weeks. He was at work as a track hand on the railroad, wheeling ties in a wheelbarrow,

in six weeks. *In this case the whole expulsive efforts of the abdominal muscles expend themselves to the outside of the incision and above the internal ring.*

I realize, of course, my temerity in bringing forward an operation of this character without further practical demonstration of its effects, and without a longer period of trial of the lasting effects than this reported case gives, but, in a certain sense, it is not an untried experiment, as there is no part of its separate procedures that has not been tried in operations of different character having different objects in view. Its point of weakness, if such exists, would be in a possible failure of proper union of the abdominal parietes; and the laparotomists have demonstrated extensively the issues to be met here, and how to meet them.

UMBILICAL AND VENTRAL HERNIA.

By Wm. H. Wathen, M.D.,
Louisville, Ky.

The importance of studying carefully the best methods of treating hernia is now emphasized because of the increased frequency of this disease following laparotomy, and especially because the improved technique in surgery makes the operation far less dangerous than it formerly was. In fact, the operation for radical cure of hernia which was until recently considered by most of the leading surgeons of this country unjustifiable because of its great mortality and the unsatisfactory results, is now, in the practice of the best surgeons, except in extreme cases, practically devoid of danger, and the patients may be permanently cured. The surgeons of the past should not be criticised because they refused to perform this operation, for until a few years ago infective peritonitis was so constantly fatal. Modern antiseptic and aseptic precautions have practically excluded this danger, and with its exclusion the patients recover from the operation, and the divided structures unite primarily and bring about a condition that more permanently resists subsequent strains. Hence, they not only recover from the operation but usually have no return of the hernia for many years, if at all.

The subject of hernia in its entirety is so vast that it would be useless to attempt, even in the briefest way, to allude to more than one or two varieties in the twenty minutes allowed for reading my paper. I will therefore confine my remarks to the preventive and curative treatment of ventral hernia following laparotomy, and the treatment of umbilical hernia.

There are many cases of ventral hernia that could have been prevented had the proper treatment been observed in the closure of the abdominal incision. To prevent hernia following laparotomy it is necessary to get perfect union by adhesion of all the layers of tissue forming the abdominal wall—the peritoneum, muscles, the deep and superficial fascia, and the skin. But especially must we get union of the layers of fascia, for unless this be done the other layers will gradually separate and hernia will follow. This cannot be done unless we succeed in bringing the cut edges of the fascia in even and perfect apposition long enough for strong union to occur. This is impossible if there is suppuration in the wound, and is generally impossible unless the several layers of tissue be separately united by the buried suture. In operations for large tumors where the abdominal walls are relaxed so that there is no tension upon the wound, all the layers may be evenly and perfectly brought together, and good results may follow by uniting the incision with uninterrupted sutures carried through the entire thickness of the abdominal wall. But, in four-fifths of the operations that are now done these conditions do not exist and there is necessarily more or less tension immediately upon the sutures. Hence, we have no assurance that the several layers are brought into apposition except it be done by separate union with the buried suture. Some operators claim that they have not had hernia following laparotomies, and that they have sutured the abdomen after any fashion that at the time suggested itself to them; but if these men will look more carefully into the subsequent history of their cases they will find that hernia is more frequent than they had supposed. I did not believe in my earlier laparotomy work that hernia would follow my operations, and I was bold in asserting that I had no hernia complications; but I now find that I have, and in some of the cases where the immediate conditions were apparently the most favorable and permanent. Of course, until recently the buried suture could not be used because of the imperfect knowledge of the best means to protect the wound against infection. This objection

having been practically overcome, almost any suture may now be buried in tissues, if properly introduced, and will not cause suppuration. It is necessary to introduce an aseptic suture in aseptic tissue and exclude it from the atmosphere. There is a precaution, however, in addition to perfect cleanliness that must always be observed in the use of the buried suture, otherwise we may have suppuration. It is next to impossible to do any operation that is *absolutely* aseptic, as there may be a few bacterial spores in the wound or upon the suture, but these are readily taken care of and destroyed by the cellular elements unless the power of resistance is impaired and conditions favorable to their development are furnished. If the sutures are drawn too tightly in all of the layers, or at any point, the normal blood and nerve supply to the part will be interfered with, the resisting powers to bacterial development weakened, and suppuration may result. In my former operations with the buried suture this difficulty was annoying, but with a broader experience I now have but little trouble in this particular. I have several cases in the Infirmary recovering from laparotomy, where the incision was closed with the buried suture; union is perfect and the tissues about the wound are nearly as soft as the other parts of the abdomen. I prefer the kangaroo tendon because it is easily made and kept aseptic, and, when chromicized, holds its integrity long enough to insure perfect union of the surfaces. I do not consider catgut so reliable unless it is prepared by some person practically familiar with the best methods of sterilization, and there is danger of too rapid absorption unless it has been carefully chromicized. The entire wound should be closed by the buried cobbler's stitch after the fashion of Dr. Henry O. Marcy, or by the continuous and blind stitch, and then hermetically sealed with iodoform collodion with a few fibres of absorbent cotton spread over the incision.

The revolution in the treatment of hernia in the last twenty-five years has been remarkable; especially is this true in regard to the treatment of umbilical hernia. It appears that the first operation in America, during this century, for

the radical cure of umbilical hernia was successfully performed by Dr. Horatio R. Storer, of Boston, in 1866. He was severely criticised by the surgeons of Boston and of this country, for attempting such a foolhardy operation. I do not think that any successful surgeon would now decline to operate upon a well-marked umbilical or ventral hernia, and it is the correct thing to operate for radical cure as soon as the hernia is well developed, for the operation is then practically devoid of danger, if properly performed, and the permanent results will be far better than if let alone until the hernia has grown large, and complications have arisen that make the operation a difficult one. One of the serious questions formerly considered was the treatment of the sac, because of infective peritonitis, when it was opened. This danger no longer exists, and in operating for radical cure of a large or old hernia the sac should be sutured at its base and removed. There is no other method of treating the sac that, from a surgical standpoint, is rational or that brings about a condition approximating the normal condition. These operations, except in cases of strangulation or extensive adhesions, are little more dangerous than an ordinary exploratory laparotomy. But, of course, great care in bringing together the layers of tissue must be observed if we want to prevent a recurrence of the trouble. Fortunately the adhesions are usually omental, which are easily separated and cause no trouble. If there are intestinal adhesions this is a complication that must be carefully managed so as not to impair or destroy the integrity of the bowel. Sometimes in fat persons the sac is filled with an omentum so large that it cannot be returned unless the ring be widely divided.

In these cases, especially if the adhesions are extensive and the omentum somewhat injured in separating them, it should be ligated in sections and removed. This adds very little to the danger of the operation, for large quantities of omentum may be removed with impunity, as I have frequently done. If the contents of the sac, when all the adhesions are separated and the recti muscles relaxed, cannot be returned into the

abdomen without too much and too long efforts, the ring of the hernia should be enlarged, but usually a little nicking at one point of the constriction will be sufficient. When the contents have been returned the thin skin over the hernia should be cut away, and the peritoneum should be resected a little beyond the bottom of the ring so as to better refreshen the edges above and to be more positive of getting an even, smooth peritoneal surface when it is united. It may occasionally be better to resect the ring also. The peritoneum should be closed by a continuous suture instead of ligation in mass.

When the sac is removed and the peritoneal edges brought together, the remainder of the operation is extra-peritoneal, and may be completed without risk of soiling or infecting the abdominal cavity. It is necessary to refreshen every part of the surfaces before we attempt to unite them, and to split the tissues, and, if possible, expose the fascia so that it also may be united. The number of layers of continuous suture necessary to close the wound will depend upon the thickness of the wall; but two or three layers will be needed in a thin wall, while four or five may be required in a thick wall. If the hernia is small and the parts come easily in apposition it will not be necessary to use any suture except those that are buried, but in fat people, with old and large hernias, it may be better to introduce several silk or silver-wire interrupted sutures half an inch from the surface of the incision down nearly to the peritoneum, to act as splints or supports to the buried sutures until firm union has occurred. In such cases, unless the kangaroo tendon or the catgut is large and thoroughly chromicized, it is probably correct to use the silk suture because it will positively not be absorbed or weakened in its resisting power until union is perfect. If the wound is entirely closed by the buried suture the best dressing is iodoform collodion as recommended after a laparotomy. These patients should be kept in bed for two weeks after the operation, with a binder tightly fitted around the abdomen; and when they are permitted to sit up, and for a year afterward, should be required to wear constantly a tightly and evenly fitted abdominal support.

In conclusion, I will report an interesting operation for umbilical hernia that I recently performed, which was not only immediately successful, but the condition of the united wound indicates that the woman will remain permanently cured. I report this case because it shows a condition as difficult to treat and as dangerous as any we will have to contend with unless in cases where there is strangulation that has partially or entirely destroyed the vitality of the omentum or the intestine.

Mrs. W., Paducah, Ky., aged fifty-three years; the mother of several children; has had umbilical hernia for about fifteen years; for several years it has been irreducible and has increased in size and caused much pain and inconvenience. On Sept. 15, 1892, she suffered intensely with partial strangulation which was finally corrected by the persistent but careful manipulation of her physician, Dr. Jewett. He devised and applied a perfectly fitted abdominal support, and sent the patient to me on September 25th. She weighs 251 pounds, and the hernia is the size of a man's head, and cannot be reduced. On the 27th I performed an operation for radical cure. The sac contained nearly all of the great omentum and the transverse colon; the omentum was firmly adherent. When the adhesions were separated the omentum was torn in many places and was so large that it was impossible to return it into the abdomen. It was ligated in sections, and a piece nearly as thick and as wide as a man's hand, and twelve inches long, was removed. The ring was so small that it was necessary to slightly nick it before the bowel could be returned. The sac was resected to a little below the bottom of the ring, removed, and closed by a continuous suture. The tissues above were widely split to expose the fascia, and the superfluous skin was cut away. The wound was closed by layers of continuous silk sutures, and several deep silver-wire interrupted sutures were used. She had no untoward symptom, and the structures in the hernial region are so thick and solid that I do not believe there will be a recurrence, though I have advised her to wear a well-fitted abdominal support for a year. Possibly heavy kangaroo tendon or catgut well chromicized would have done as well as silk.

THE CURE OF INGUINAL HERNIA IN THE MALE.

By Henry O. Marcy, M.D.,
Boston, Mass.

Until recently the cure of inguinal hernia in the male has been considered at the best accidental, and when apparently effected, generally doubtful, and it is still taught that the hernia is liable to return.

The great majority of surgeons look upon an attempt at cure as ill-advised, and believe that operative measures are not to be undertaken except in cases of strangulation.

There is abundant reason for such conclusions, when judged from the earlier history of surgical procedures as attempted for cure. It is impossible in the limit assigned me to consider at all exhaustively this most interesting subject. Three centuries ago the problem was mastered by the surgeons of Europe, but only by the sacrifice of the testicle and the removal of the cord. In this way the abdominal opening was closed and the resulting cicatrix was unyielding. The demand for relief from this burdensome complaint was, however, so great, even at this sacrifice, braving the suffering from surgery without anæsthetics and with the slow healing of a dangerous suppurating wound, that the operation was forbidden by royal edict, lest the nation should suffer from the lessening of the reproductive power of the race.

It is necessary to revert briefly to the anatomy of the parts involved, in order to note the method by which Nature permits the passage of the cord through the abdominal wall without pressure, and retains without inconvenience or suffer-

ing the abdominal organs. This passage is normally lined with peritoneum, loosely attached to the abdominal wall without, and to the cord within, the canal. This peritoneum, owing largely to its loose attachment, presents a slight infundibuliform depression at the site of the internal ring, but never more than to permit a very limited movement of the cord within the canal. The canal itself traverses the abdominal wall obliquely from without inward, in such a direction that the intra-abdominal pressure, radiating from a centre represented by the promontory of the sacrum, is distributed equally along the line of the canal, compressing its walls laterally. The disposition of the canal is not unlike that of the ureter in its entrance into the bladder, the intra-vesicular pressure, closing yet the more firmly the ureter at its orifice. The pathological changes incident to acquired hernia present, as the first factors, a bulging of the infundibulum of the peritoneum about the cord and a depression of the lower border of the internal ring. Thus is produced a change, at first slight, of the direction of the intestinal wave impulse which acts at times as a wedge to open yet more the ring. Little by little the axis of the canal changes, until at length the intra-abdominal pressure is in the line of the opening instead of at right angles to it.

These changes, which culminate in a well-pronounced hernia, more or less direct, have long been recognized, but it of necessity remained that the reconstruction of the canal to its normal conditions was impossible, until aseptic surgery established the methods of primary repair of the tissues upon a scientific basis. The most important factor, as I shall presently demonstrate, is the closure of the deep structures by means of the buried animal suture.

The essential considerations for the cure of hernia are :

First. *Strict aseptic conditions.* These pertain alike to all modern surgical procedures, and need not be recapitulated to this audience.

Second. *A free dissection.* This is necessary in order to lay bare the internal ring, to permit of the enucleation of the

peritoneal sac and the separation and elevation of the cord out of the wound. The external epigastric artery often courses in the line of the incision. It is not seldom that the size of this vessel is such that the operator fears he has wounded the larger vessel.

Third. *The disposition of the sac.* The separation of the sac to its very base before removal is to be recommended as the rule. There are times, however, when it is not easy to free the peritoneal pouch, owing to adhesions to the surrounding tissues, and in large, old, irreducible hernia, a more or less intimate fusion of the contents to the inner wall of the sac.

It is generally better to open the sac before ligating or sewing through its neck, since, by so doing, the condition at the internal ring is assured, and by such knowledge the operator is often profited even when the sac is completely empty. Not seldom the omentum is adherent at the internal ring, and even a constricted loop of intestine may escape observation when it is attempted to resect the sac unopened. Freed to quite within the ring, tension is to be made upon the sac, and then the sutures are applied in the line of the long diameter of the internal ring, and the sac is resected near its base and excised. The retraction should be sufficient to carry the resected peritoneum quite within the ring.

Mr. Macewen and his followers who recognize the (necessarily) from within outward funnel-shaped opening of the inguinal ring after operation, make the attempt to utilize the freed sac, in whole or in part, by puckering it up into a mass and with it embossing the internal ring, more or less filling the open space, otherwise left to invite a lodgment of the abdominal contents and wedge open the canal, causing a return of the hernia. Having demonstrated the feasibility of reconstructing the obliquity of the canal, it needs no argument to show the greater value of such a procedure over the substitution of plugging the opening with pathological tissues.

Fourth. *The posterior border of the inguinal canal.* Having freed the cord to its point of entrance within the

abdominal cavity, and lifted it to one side, we are prepared to study the structures which may be utilized in the re-formation of the internal ring and the posterior border of the canal. Sir Astley Cooper was the first to emphasize the remarkable development of the transversalis fascia, which is here usually found to consist normally of a thick layer of connective-tissue fibres.

In large hernia the lower border of the internal ring has often fallen quite on a line with that of the external ring—direct hernia—but it will usually be observed even where abnormal pressure of the truss has produced absorption, that Nature made a distinct effort to fortify the parts, and that the lower margin of the ring and the transversalis fascia have become markedly thickened. These structures are to be utilized in the re-formation of the posterior wall of the canal. Upon the lower and outer border this fascia blends with the posterior edge of Poupart's ligament, while upon the upper and inner border it unites with the lower edge of the transversalis muscle and conjoined tendon. Often the finger may be introduced into the internal ring to aid in the guidance of the needle, which is made to traverse from side to side the relaxed fascia and evenly intra-fold it with a layer of double continuous tendon sutures. These are carried from below upward until the internal ring is closed upon the spermatic cord at its exit from the abdominal cavity. One unaccustomed to the operation will be surprised to note the amount of tissue that may be thus intra-folded to form the posterior border of the canal.

If, however, for any reason the structures seem to be insufficient, it is easy to unite the deeper edge of Poupart's ligament to the lower border of the transversalis muscle, and thus strengthen the parts. This is usually recommended by Prof. Bassini, and upon this, in large measure, his method of operation is based.

Halsted, of Baltimore, has gone one step farther, and buttresses the posterior wall of the canal by uniting all the muscles beneath the cord, forming an entirely new canal external to

them. These operators carry the methods which I have long used and taught, to extremes which I have found rarely necessary, very likely in large measure because, by my method of using the deep double continuous suture, I am enabled to coaptate a thick layer of firm resisting structures and re-form the posterior portion of the inguinal canal much more closely after the pattern of normal construction.

Once satisfied that the coaptated structures are sufficient, the cord is replaced and the uniting of the external structures is conducted in the same manner, with a layer of deep double tendon sutures joining the divided muscular wall of the abdomen and bringing into close apposition Poupart's ligament with the conjoined tendon quite upon the cord, until the external ring is reconstructed. It is surprising to note the little interference which follows upon the close approximation of the structures upon the cord the entire length of the canal, as evinced by back-pain, swelling of the testicle, or even œdema of the scrotum.

The structures external to the muscles are best approximated by one or more layers of single continuous sutures taken by means of a Hagedorn needle introduced from side to side. The skin is closed in a similar manner with a continuous buried tendon suture. The needle, straight or curved, is best held in the fingers, and is carried through the deep layer of the skin, entering upon the other side of the wound just opposite the point of emergence of the last stitch. This is important, otherwise the wound assumes a wavy look, owing to a puckering of the skin, as tension is made upon the suture.

This I have termed "the *parallel suture*," since the needle is carried through the skin exactly parallel to the line of incision. It will be noted, however, that when the points of entrance and emergence of the needle are exactly opposite the suture lies in the wound transversely, and if, for the purpose of demonstration, the wound is allowed to remain only partially closed, the suture lies in parallel lines, like the

rounds of a ladder, and at right angles to the long axis of the wound.

I believe it is generally safer to conduct the entire operation under irrigation with a weak sublimate solution, and often not a sponge is needed. It is better to remove shreddy tissue, if much tearing of the parts has occurred, since it is most important to join well vitalized structures in order that primary union may supervene. I think it is also wise to dust the parts with iodoform before sealing the wound with collodion.

It is well worth while to take the time and trouble to dry the wound, and to coaptate evenly the edges of the skin, sealing, if need be, only a part of the wound at a time. This is best effected by spreading evenly a few fibres of absorbent cotton upon a smooth surface, and moistening with iodoform collodion. This, gently laid upon the approximated edges of the wound, soon dries and makes a germ-proof dressing, holding, as in a splint, the tissues in a firm, sure grasp. The treatment of wounds in this way is of sufficient importance to be emphasized, since primary union almost always supervenes, without suppuration, pain, or inflammation.

Aseptic animal sutures, aseptically applied in an *aseptic* wound, *aseptically* maintained. This is the *sine qua non* without which more or less of failure must ensue. Otherwise buried sutures are not to be commended in any wound, and must ever be a source of danger. The advantages of the above method of operation and closure of the structures for cure of hernia are obvious. First. By no other means than deep closure of the parts by the use of sutures to be left in the wound, and not to be subsequently removed—buried sutures—can the posterior border of the inguinal canal be restored, the parts strengthened and reinforced, or even the neck of the sac closed, and the redundant tissue removed. To accomplish such a purpose the use of silver wire or silkworm-gut is not to be considered, since, by general consent, these sutures must act as irritants, and usually are a source of much annoyance and suffering until they are finally eliminated as foreign bodies. Silk is used by many operators for this

purpose, but abundant clinical and experimental studies have demonstrated that silk at the best is encapsuled, never absorbed. It may, after months of suffering, be thrown off by suppurative processes.

A wound that is closed aseptically in layers with the tendon suture leaves no pockets to become distended with blood or serum, and hence the drainage-tube is not required. In aseptic wounds the drainage-tube is ever to be considered as a foreign body, positively harmful, since it separates tissues which should be in contact, and, if long maintained, this portion of a wound must be restored by secondary processes. During its retention it is ever a positive source of danger from infection, which must be guarded against by the most careful of antiseptic dressings. This is most difficult in the region of the groin, and the large proportion of suppurating wounds following operations for hernia, in the hands of our best operators, shows the great risk from an open wound in this portion of the body. The complete closure of the skin by a line of buried animal sutures has been my daily practice for years, and was decided upon in order to prevent stitch abscesses long before the beautiful demonstrations showing their cause to be from the introduction with the suture of the *micrococcus pyogenes albus*, the normal habitant of the healthy skin, aiding in the destruction and proliferation of the dying and dead epithelium.

The iodoform collodion seal is useful in two ways: First, it holds in fixation at rest the divided edges of the skin which should be carefully approximated; secondly, it prevents the possibility of subsequent infection. An aseptic wound thus protected must remain aseptic, and, when the tissues are well vitalized, union is always primary. If the skin is in close approximation, the union is linear, and often after a few months can scarcely be detected.

In many wounds in other parts of the body this is of great value, especially facial wounds in the female. It is interesting to note briefly the repair-processes which supervene in such a wound. Histological investigations upon animals

show that the tendon or animal suture is little by little invaded by leucocytes which are abundantly proliferated to surround the material. Gradually, dependent upon age, activity of developmental processes, vitality of structures, etc., this proliferated material is transformed into connective-tissue cells, until the suture is, in large measure, replaced by a band of living tissue. This is important in most wounds, but is perhaps never of greater value than in hernia, where the resistant power of the structures has long been defective.

An aseptic wound thus closed is rarely painful, œdematous, or even tender to gentle touch. The new proliferated material can be easily felt for a considerable period, and is perhaps at its maximum from four to six weeks after the operation.

For many reasons the tendon suture is greatly to be preferred to catgut, a subject of extreme interest and of the first importance, but can receive here only reference. The kangaroo tendon has met with the unqualified approval of nearly every operator who has used it, and is now in the market in quantity and price so that it comes within the reach of every surgeon. Time does not permit an analytical comparison of the different methods of operation. I have recently treated this subject *in extenso*.[1] The operation is eminently a safe one. In a table which I have recently collated of over three thousand cases, the mortality is less than one per cent., and is usually explained as having been from causes which the authors state were accidental, and were not due to the operation. In numbers Bassini's clinic leads the list, 262 operations and only one death, due to pneumonia; Championnière reports 254 cases operated upon, 2 deaths; Schede's clinic, 165 operations, 2 deaths; Banks, 106 operations, no deaths; Park, 115 operations, 85 reported cured; Marcy, 115 operations, no deaths, 78 traced, 4 relapses. The experience of the late celebrated John Wood, of London, covering years of

[1] Anatomy and Surgical Treatment of Hernia. By Henry O. Marcy, A.M., M.D., LL.D., Boston. Quarto, with 66 full-sized illustrations; 8 colored, 37 woodcuts. D. Appleton & Co., 1892.

labor before the period of antiseptic *régime*, gave only seven deaths in 339 operations.

By estimate, between three and four millions of the people living in the United States are subject to this usually life-long progressive disability; and, if the demonstration is complete, that the risk of life is less than one per cent. from the operative procedures instituted for cure, and that scarcely more than ten per cent. are subject to relapses, and these almost invariably in a state improved by the operation, the plea is a very strong one to consider favorably the advisability of operation in a very large majority of all the sufferers from hernia.

We are all painfully familiar with the dangers of strangulation in neglected hernia and the large percentage of mortality which results therefrom. It seems to me a duty, resting heavily not only upon all surgeons, but upon every physician residing where he cannot easily obtain surgical assistance, that he make himself familiar with all the details of the operation for hernia, its relief when strangulated, its cure when troublesome, and thus fit himself for the discharge of a solemn obligation which may at any moment of his professional experience be freighted with the issues of life or death.

DISCUSSION.

Dr. V. P. Gibney, of New York.—I do not know why I should be called upon to open a discussion on the subject of hernia. I presume it is because I happen to be surgeon-in-chief to the hospital for the relief of the ruptured and crippled. I may say, in passing, that my treatment of ruptures forms a small part of my work at the hospital. I have learned enough about the treatment of ruptured people to get competent men to look after that subject, and the gentleman who attends to that work is Dr. Wm. T. Bull. While I have very little to do with ruptured patients personally, I have an opportunity of seeing the results of a great many cases that have been operated on in New York and its vicinity, and I do not know of a better school for studying the final results of the radical cure of hernia. The hospital is looked upon somewhat as a dangerous place for cured (?) cases, and the

surgeons of New York are constantly asking me whether we have kept any of their final results or not. Dr. Bull has been collecting these final results for a number of years. I have learned this much about the operations which have been regarded as radical cures in the true sense of the term, that they are followed sooner or later by relapses, which of course we did not expect. Formerly we were taught that the radical operation was to be recommended in cases that were not manageable by a proper truss. The advice is given to patients now to have an operation done, and that if the hernia is not cured radically, it can be held by a properly fitted truss. The views are changing somewhat during the last few years. Some of the difficult cases to hold with trusses are relapsed cases, and without any discredit whatever to Dr. McBurney, these cases have relapsed after the McBurney operation. The tissues are very much relaxed, and it is almost impossible to get a pad which will hold the hernia. Then our friends, the laparotomists about New York, contribute in a measure, also, to our out-patient department, and a number of cases of ventral hernia come for support, and we have to care for them. Just what the proportion is I cannot say. A few days since, at a meeting of the Surgical Society of New York, Dr. Fowler, of Brooklyn, read a paper entitled "Observations in the Radical Cure of Hernia." In the discussion practitioners were called upon to give their final results so far as they knew them. The cases were mostly hospital ones. I do not know what the results are, as I have not time to look them up. It was assumed that because patients did not come back to them for treatment they were cured. I fear many of our statistics from which we drew our deductions are cases that have been imperfectly followed, and for that reason I feel that a discussion on this subject by such eminent men as are present on this occasion ought to be prolific of great good. I want to urge you to be careful in your operations to get the parts closely approximated and to look after the wounds for a long time after they have healed. The results that have interested me most in the last year or two are the operations on children. We have been taking into the hospital young children whose herniæ have not been well cared for by trusses, and there have been a great many extensive adhesions of the sac. These were dissected. Twenty-five cases have

been operated on within the last few months. Only one out of twenty-five had primary union, and they are now going about with very slight trusses. Dr. Fowler in his paper does not believe in the use of trusses for the cure of hernia, Dr. Bull always uses trusses, while Dr. McBurney does not care much for them. Opinion is so divided on the subject at the present time that I think it is going to take many statistics to convince us one way or the other.

Dr. Howard A. Kelly, of Baltimore.—My own largest experience has been with ventral herniæ, both umbilical and those occurring in the scar of the incision following an abdominal operation. These latter herniæ are almost always due to imperfections in our treatment of the abdominal wound, to imperfect approximation of its various layers, which ought to be brought into accurate apposition immediately after the operation. The commonest mistake is that of trying to take too many layers of tissue in the grasp of one set of ligatures, for very often one or more of these layers will slide above or below the other, thus leaving a weak place in the wall where hernia may occur.

One of the most frequent causes of hernia is the glass drainage-tube, which is to be looked upon as a confession of weakness and imperfection in our technique. The longer the tube is left in, the greater is the danger of subsequent hernia. This is easily understood by observing the way the drainage tract heals; the surfaces of the wound touched by the tube become glazed over and begin to contract and form a scar. The skin thus approaches the peritoneum on each side and the muscles and fasciæ recede from the wound, so that when the tube is taken out two narrowed surfaces come imperfectly together, leaving this spot ever after the weak point in the abdominal wall. For this among other reasons I have almost entirely discarded the use of the glass drainage-tube in my practice. Herniæ are now rare occurrences. In my earliest work I saw many of these cases.

The operation for these herniæ is difficult, but one of the most satisfactory procedures if properly performed. I can only mention here a few of the principles. In the first place, let me insist that among umbilical herniæ there is a certain small class of cases, found in fat women, in which we ought never to attempt to cure the hernia radically. On the contrary the proper method

of treatment where the hernia has become strangulated is precisely the opposite to an attempt to make a radical cure obliterating the sac; it is to make the hernial sac so large that all the intestines can enter it without danger of strangulation, thus rendering it harmless. One of my first surgical experiences dealt with a striking case of this sort. A stout, elderly midwife was several times admitted to the surgical wards of the Episcopal Hospital of Philadelphia when I was resident. She had an incarcerated hernia which at times became strangulated. She refused operation, and nothing more than rest and cold applications were adopted. She continued under my care after I left the hospital, and in one of these attacks finally died. A post-mortem examination revealed the fact that a great portion of the intestines lay outside the abdominal cavity within the sac. The caput coli and vermiform appendix were there displaced up into the left hypochondriac region over the spleen. The abdominal cavity had accommodated itself to the size of the few viscera left in it, and was so contracted that even after death it was well nigh impossible to force the viscera back again into the abdomen. The proper plan of procedure for the relief of this woman would have been to lay open the sac and cut though the lower margin of the sharp ring forming its neck downward in the linea alba, in this way so increasing the capacity of the sac that strangulation would be impossible. I know of a case occurring since, where a colleague had similar conditions to deal with, and he forced the contents of a large hernial sac back into the abdominal cavity, and then with difficulty overcame the tension of the abdominal walls and closed the incision. This patient vomited excessively until she died in a few days.

The danger of a hernia is in the ring at the neck; cut this and it is no longer dangerous.

In almost all cases, however, we can secure a radical cure by bearing in mind certain principles. In the first place, all of these sacs are limited by a sharp ring in the abdominal wall; it is here the strangulation occurs. In performing an operation, however, for the cure of hernia by obliterating the sac it is not sufficient to consider the margin of this ring as its outer limit, and then to denude and bring the edges of the ring together. It is absolutely essential to cut out the dense fibrous tissue of the ring, and

to go beyond this into the sound muscular and fibrous tissue, and to utilize this in the closure.

Note also that in the median line above and below the ring the muscles and fasciæ are farther distant from the margin of the ring than they are at its sides. Your denudation, therefore, will be in the form of a long ellipse extending vertically. After making a good denudation all around *into sound tissue*, the next essential point is to apply your sutures so as to secure good union. This may be done in the following way: First, close the peritoneum by a continuous catgut suture, second, pass interrupted silkworm-gut sutures one-third of an inch apart including skin, subcutaneous tissue, fasciæ, and muscles down to the peritoneum. If now these silkworm-gut sutures are tied at once the whole wound will be closed, it is true; but we cannot be sure that the muscles and fasciæ will remain in accurate apposition until they unite firmly.

This important point is therefore secured by uniting the muscles and fasciæ with a line of buried catgut sutures, before tying the silkworm-gut sutures, and after which they are to be drawn up and tied. In this way the silkworm-gut sutures act by carrying the tension, while the catgut sutures serve to secure accurate apposition. The catgut alone also will not suffice, as it softens and gives way sooner when there is tension upon it, and in three or four days the fasciæ and muscles would fall apart. This is prevented by the silkworm-gut.

Dr. Marcy's work in the line of the radical cure of hernia in the male is one of remarkable merit, and we must acknowledge that in this field he is *facile princeps.* It is particularly pleasing to note that Dr. Marcy accords American surgeons their full credit. There is, as a rule, on the part of my countrymen, too much of a desire to go abroad in the citation of their authorities and references. While I do not myself treat hernia in the male, I have seen a number of cases operated upon, brought before the Johns Hopkins Hospital Medical Society by my colleague, Dr. Halsted. The results obtained by Dr. Halsted's operation are remarkably successful. The principle point in the operation consists in the transplantation of the cord above the inguinal canal, and an obliteration of the canal below this. All sutures are buried; even the skin sutures are subcutaneous.

Dr. Baxter's operation is similar, I think, in one essential element of success—in the transplantation of the neck of the sac. In all hernial operations primary union is an essential element of success.

I have but one objection against Dr. Marcy's technique. I would urge him to abandon the use of solutions of the bichloride of mercury. The value of the inhibiting power of this drug over germ life is more than counterbalanced by its necrosing effect upon the tissues.

I have several times operated upon inguinal hernia in the female when the hernia chanced to complicate a pelvic tumor. The operation under these circumstances is simplified by the abdominal incision in the median line, for after opening the abdomen a finger can can be readily introduced into the inguinal canal on the inside, assuring the absence of the bowel and guiding the knife in cutting down upon the sac and denuding the pillars. I obliterate the sac and closed its walls by suturing in the manner just described for umbilical and ventral herniæ.

Dr. Horace T. Hanks, of New York.—It gives me great pleasure to be present and take part in the discussion of the papers which have just been read. Like the gentleman who have just preceded me, Dr. Kelly, I shall confine my remarks to herniæ in the median line of the abdominal wall. My work has been almost exclusively in the department of diseases of women, having been for fourteen years connected with the staff of the Woman's Hospital in New York. I have often been called upon to operate for umbilical hernia (strangulated). During the past five years we in New York—of course, such accidents never occur in any other city (?)—have been flooded with cases of ventral hernia following cœliotomy. And gentlemen, you would hardly believe it possible, but I certainly believe that 25 per cent. of all cœliotomies which were performed up to four years ago—25 per cent. of all these patients who lived three years—have had ventral hernia. Three years ago I presented a paper at the meeting of the American Gynecological Society on this subject, urging more care and skill in closing the abdominal wall, especially insisting upon the absolute necessity of bringing the strong fascia or sheath of the rectus muscle in exact apposition by independent buried animal sutures. At that

time I hardly realized that so many good operators had had this accident. But later I learned from Dr. Starr, the house surgeon of the Hospital for the Crippled in New York, and Dr. Gibney, who has just spoken on this general subject, will remember him as his most excellent assistant, that there had been at his hospital over fifty applicants for trusses for the relief of ventral hernia in one year. Remember that this means fifty of the very poor patients, and certainly would not include more than one-third of all cases. This accident, therefore, is a common one, and after pledging ourselves to take time to sew up the abdominal wound methodically, in all our future work we will apply ourselves to the cure of these cases which present themselves to us.

There are the large and small herniæ. The former are troublesome and often dangerous, the latter may be simply a source of annoyance. The large umbilical herniæ are always, when strangulated, best dealt with by opening into the abdominal cavity below the umbilicus; then, with finger passed from within outward through the ring, pass the blunt bistoury on the finger, using the finger as a grooved director. Then cut from the ring towards the pubis until the abdominal incision has been reached. The way is then cleared for a careful and thorough inspection of the hernial ring and the intestines and omentum; every portion of the latter, which in old cases will have grown in all directions into the adipose tissue just under the skin—all of this unhealthy omentum must be dissected off, the intestines examined, and, if healthy, returned at once to the cavity.

The peritoneum should then be closed. The best method for closing the peritoneum in these hernial cases is always to use absolutely aseptic animal ligatures, making a seam of the two edges; then, pushing this seam downwards into the cavity, take a few Lembert sutures a full half-inch on each side of this sunken seam, and this rib seam will be pushed into the cavity and will act as a rib, or strong ligament or buttress to keep the intestines and omentum from attacking and pushing through this immediate locality again. Then close the fascia, which is always firm here, with catgut or tendon sutures; then the integument in a similar manner. If not certain about your catgut lasting, put in two, three or four silkworm-gut sutures through all the different layers of the wall. In a small fistula following the use of the

drainage-tube little annoyance will follow excepting from the necessity of cleaning the parts. After waiting for six months, if they do not close spontaneously, then give an anæsthetic and examine for a ligature. No instrument is so useful in this work as a small Sims curette, the cutting edge of which has been filed like delicate saw teeth. Any ligature can be removed with this instrument if it is in the sinus. If the sinus has been closed and the plastic material has been absorbed which surrounded the track in the abdominal cavity, and the intestines or omentum protrude, then, under ether, cut down directly over the fistula, enlarge the canal, push back the bowel or omentum with the handle of a tenaculum and scarify. Then sew up with a full curved sharp needle, using an animal ligature, beginning with the tissue just above the peritoneum and ending with the integument. For large ventral herniæ, after proper preparation, cut out the old cicatrix carefully, then as carefully dissect down to and cut through the peritoneum, examine for adhesion of the intestines, fold in the peritoneum as I have just indicated in umbilical herniæ, then be sure and bring up the recti muscles, and sew them. Then as surely bring the fascia or sheath as carefully together, the skin in the usual manner.

The importance of bringing the muscle up under the fascia has not been well understood, I am certain. We want absolutely no dimples, no soft spots, no unevenness in this line of the wound. None but a careless operator need ever fear an accident in this work if he does it aseptically after the manner I have described.

Dr. James T. Jelks, of Hot Springs.—I was very sorry that Dr. Wathen did not tell us how we should relieve cases of ventral hernia. If we will bear in mind that we have an oblong situation here, and that is quite difficult to bring the two recti muscles together with sutures, it seems we should have some relaxation of the tissues; in other words, to prevent or cure ventral hernia the fascia of the recti muscles must be brought together. In ventral hernia the muscles are separated in the centre, and in bringing them back there is a great deal of tension, and we fail to get union. If we take a knife and split the fascia of each muscle on its anterior surface we loosen it and get rid of the tension. We should split it long enough to get below and above the ring to catch the set of tissues which you can

approximate with sutures. There is no other way that I know of to bring the recti muscles together without a great deal of tension.

Dr. A. V. L. Brokaw, of St. Louis.—I desire to call the attention of this Association to a point which I think is a mistake on the part of Dr. Wathen. He speaks of a sac in ventral and umbilical hernia. It has been my fortune to have met with several cases, and I have repeatedly operated for such conditions, but I have never found a well-defined sac in any of my cases. We find nothing but integument, with a little fascia below it. The integument is so thin that it is inseparable into layers. I think he has confounded ventral hernia with inguinal hernia, which has a well-defined sac. I would like to have Dr. Wathen explain the point he makes of separating the sac when no sac exists such as he describes.

Dr. Edwin Ricketts, of Cincinnati.—There are two points that have not been referred to in the discussion in the treatment of ventral or umbilical hernia. The first is in regard to the manner of making the incision. I think when the abdomen is opened and much attention paid to opening in the linea alba, that these are the cases in which we are more liable to have hernia make its appearance. We should make a clean cut and pay but little attention to the linea alba. Even if we cut through one of the recti muscles on either side of the linea alba we have a better chance for a cicatrix than in bringing the recti muscles together without splitting the fascia, as suggested by Dr. Jelks.

Another thing: in regard to adjusting the sutures in these cases. Quite frequently the sutures are too many, and are often tied too tightly. By attention to these two measures, which are on the preventive line of hernia, I believe we can accomplish more than we can by trying to cure the disease after it makes its appearance.

Dr. W. E. B. Davis, of Birmingham, Ala.—I desire to call attention to a point referred to by Dr. Kelly. If I understood him correctly, he said that in cases of very large ventral hernia, where the intestines have become attached to the sac, and where it is impossible to reduce them without great tension, we should not operate.

Dr. Kelly.—I had reference to those cases of large ventral hernia where the extra-abdominal contents were such that it was impossible to reduce the hernia and to get the structures back without extreme tension.

Dr. Davis.—Cases of hernia that cannot be reduced generally call for operative procedure. I think it would be much safer in some cases of the form described to incise the intestine and get rid of the gas. One of the saddest cases I have ever seen was a strangulated hernia of this class which had been neglected.

I cannot see the wisdom of splitting the fascia in order to bring the recti muscles together, as seggested by Dr. Jelks. If we are going to split the fascia we ignore all argument in favor of bringing it together. Fascia split in order to bring the abdominal wound together exposes the injured part to hernia. We would have the risk of three herniæ instead of one.

Dr. Jelks.—I mean the external layer of the fascia.

Dr. Davis.—I have been of the opinion for some time that it is better to make an incision into the abdominal cavity through the muscle and not in the linea alba. We are thus less liable to have hernia.

Dr. Bedford Brown, of Alexandria, Va.—I do not desire to make any allusion to the operative procedures in cases of hernia, because that phase of the subject has been thoroughly and exhaustively discussed. I simply desire to make some allusions to the treatment of peritonitis following hernia, a complication that may arise with all of us. Doubtless all of you who have operated on hernia to any extent have had that complication. I have had it myself. I desire simply to cite the history of a case, and the citation of that case will cover the whole ground in regard to the treatment of peritonitis from this cause. A year or so ago I had a case of strangulated hernia in a female fifty years of age. The case was irreducible, and had been so for several years. When I saw the case it had been strangulated for forty-eight hours or more. I operated, and found it was an omental hernia, and the omentum that was constricted was gangrenous. I tied the gangrenous portion above the line of gangrene with a ligature, and I cut off the gangrenous portion. I supposed I had removed all gangrenous tissue, put it back, closed the wound, and the whole procedure was antiseptic. At the end

of twenty-four hours peritonitis supervened. There was pain and tenderness in the abdomen, rise of temperature to 104–5°, pulse 130, tympanites very great. I reopened the wound, took out the sutures *in toto*, opened the inside and edges of the wound, opened the canal, took a No. 12 soft catheter, inserted it in the peritoneal cavity; I then connected with that a fountain syringe. I washed out the peritoneal cavity with boiled water at 110°.

I did that not once, but many times, and during the irrigating process the ligature that I had applied came away, as well as portions of gangrenous tissue, showing that the gangrene extended above the ligature. The fever began to decline, tympanites likewise, the pulse and temperature reduced, the woman was more comfortable, the irritability of the stomach began to cease, the whole thing began to undergo a process of subjugation, and finally the peritonitis was absolutely subdued. During this process the wound was suppurating, then it began to granulate, and finally the peritonitis ceased entirely, the wound healed, and the case resulted favorably.

Now I am satisfied that in cases of gangrene and peritonitis, or peritonitis without gangrene, following operations for strangulated hernia, the best treatment is to reopen the hernial wound *in toto*, insert a flexible tube up through the ring into the peritoneal cavity, and practice irrigation with simple warm water so as to keep that cavity clear of all débris, pus, or other septic matter.

Dr. J. McFadden Gaston, of Atlanta.—My experience has been very limited in operating for hernia, but the remarks of Dr. Brown encourage me to present an idea which I have been inculcating in my clinical lectures for a number of years, and that is, with a view to the relief of inguinal hernia, to adopt exactly the same process we do in operating for strangulated hernia. I would like for the essayists, in their closing remarks, to tell us what proportion of cases they have operated upon in strangulated hernia have returned? My plan has been to bring the edges of the sac together, stitch them from side to side, and in this way the canal is almost obliterated. Turning the sides down you get the wall very much stronger. You necessarily, in strangulated hernia, carry this up to the internal ring, and my

idea is that this very simple, old-fashioned procedure is a most effective operation for inguinal hernia. I do not say that it is a radical cure for it.

Dr. A. M. Cartledge, of Louisville.—The aspect of this subject that most interests me is the prevention of ventral hernia after abdominal section. I am a little surprised that there are not more advocates of the usual method of closing the abdominal walls. I say *usual method*, because I believe there are many gentlemen present whose practice it is to embrace all the structures from the peritoneum out in one set of interrupted sutures, and I further believe their results are good, so far as the prevention of ventral hernia is concerned. In order not to be misunderstood, I want to say that I am a strong advocate of the buried suture applied elswhere. I think it one of the greatest additions to modern surgical practice.

Certainly there must be something radically wrong in the technique of some men when we hear such varied expressions as to results obtained from the use of interrupted sutures which traverse the entire wall. We were told by distinguished gentlemen present, who have had large experience in this work, that it is essential to approximate the different layers separately. While I have nothing to say against such a procedure, I think there are many surgeons who are closing the abdomen by a shorter method that keeps patients on the table less time and with comparatively few or no herniæ resulting. What is the trouble? Our personal experience is our most valuable guide. In my early career I had one or two ventral herniæ following abdominal section.

I believe they were due to a defective application of the interrupted suture. Now I have no herniæ following my operations. I use interrupted silkworm-gut sutures, with very occasionally separate suturing of the peritoneum with catgut. I place the sutures now much closer than I formerly did, exercising great care to see that the fascia is not retracted and thus failure to embrace it in the suture. One of the preceding speakers has said that in a great many cases we tie them too tightly; there is a right position in this matter. I believe the usual error is in placing the sutures too far apart; we should not have them over one half inch. Dr. Kelly has referred to the drainage-tube as

being provocative of ventral hernia; that has not been my experience.

I think we must look for the cause elsewhere. I do not think that hernia occurs with an undue frequency at the point of introduction of the drainage-tube, but as a rule above this point. We know that lymph surrounds the drainage-tube as early as the third day, and that in most cases the peritoneal cavity proper is cut off from the drainage-tube tract; but I do not believe the drainage-tube properly managed is of itself a source of danger or causative agent in the production of ventral hernia. I have not reached that stage of perfection in my operative work that I can dispense with the drainage-tube. In brief, I believe the causes of ventral hernia are to be found in improper apposition, the intervention of blood clots between the wound, and stitch abscess.

Dr. Cornelius Kollock, of Cheraw, S. C.—I would like to ask if any member of the Association has ever had any experience in reducing strangulated hernia by aspiration without further operation? I have reduced it several times in that way. I reduced one a month ago with a hypodermatic needle. I have never found any difficulty in reducing strangulated hernia by aspiration, except in cases in which adhesions had formed, or where a mass of omentum was packed between the intestine and the walls of the canal.

Dr. W. E. B. Davis, of Birmingham, Ala.—Dr. Kelly spoke of the danger of stitch abscess, that where the stitch hole passes through the peritoneum there was a great deal of danger of pus getting into the cavity through this opening. I would like to ask him, if by the time pus could form, in forty-eight hours, if the peritoneal cavityd woul not be sufficiently protected. I know in my experimental work I have found the cavity shut off in twenty-four hours, and it must be a rare thing for pus to get into the abdominal cavity, from this source, with ordinary care.

Dr. Kelly.—This has been a matter of demonstration in the pathological department of the Johns Hopkins Hospital. Dr. Councilman has shown post-mortem that pus discharged itself through the stitch wound; therefore, if I had one case out of fifty that discharged pus into the peritoneal cavity I should want to change my method.

Dr. L. S. McMurtry, of Louisville.—I am very much interested in the discussion, and what experience I have had in abdominal surgery is in favor of the interrupted suture and of the use of the drainage-tube. I have done a large number of abdominal sections, and of the entire number I have not known of one post-operative ventral hernia. In nearly all of my cases I have used a drainage-tube as a routine measure. We have in the last few years been endeavoring to improve the technique of abdominal surgery, in which work Dr. Kelly has played an important and conspicuous part by his researches in this line, and hence it is with a great deal of deference that I disagree with him. We have striven to reach an ideal state, and the assertion made by one of the speakers, that in using the drainage-tube it is a confession of imperfect technique, I do not agree with at all. I think surgery is not to be placed upon the basis of mathematical or anatomical demonstrations, but an ideal technique in any department is that which insures the greatest safety to patients and saves the greatest number of lives.

In the point under dispute I do not see how we can gain anything by sewing separately the peritoneum in closing the incision in an abdominal section. The peritoneum, if the sutures are placed well over on to the edge, will wall itself off in twenty-four hours. We consume a good deal of time on the operating table, and in operations of emergency and magnitude, in doing an ideal method of suturing, and it is entirely unnecessary. By using interrupted sutures after the manner described by Dr. Cartledge we get just as good a line of union. The use of the drainage-tube does not complicate the case in the least. I believe the convalescence after abdominal section will be better and more prompt where we use irrigation in the majority of cases in preference to prolonged sponging.

In regard to the question which Drs. Davis and Kelly have been discussing concerning stitch abscesses breaking into the peritoneum, I think it is very exceptional that these cases occur. We know the tendency of internal suppuration to come to the surface. Pressure inside of the abdomen tends to bring everything toward the surface.

Dr. Baxter, of Chattanooga.—I desire to say a few words in relation to sutures. I take it that in the closure of the abdominal

wound outside of the recti muscles the real cause of failure of direct homologous approximation is in the compressible sub-peritoneal fat.

In regard to the remarks of Dr. Jelks with reference to the incision through the superficial fascia covering the rectus muscle, I have seen several operations that went through the rectus muscle, and in every case I have seen the fascia has contracted backward, so that there was a failure of close approximation of the superficial fascia. I could hardly tell what became of the lower layer, as it did not come in direct approximation. There was approximation of the muscle, but not of the fascia. I reached this by an incision through the fascia. You will see it contract, and more or less of the rectus muscle. If this is true he weakens the wall when he incises that in order to bring about approximation in the linea alba.

I regret that the discussion has turned almost exclusively to ventral hernia. I wanted to know more about inguinal hernia.

Dr. Marcy, of Boston.—I am deeply interested in the remarks of Dr. Gibney. I have watched with care the operations and the results reported during the last few years by the surgeons of New York. In a recent work I have carefully revised them.[1] I think there is abundant reason for the imperfect results as reported by Dr. Gibney. He has referred to Dr. McBurney and his methods of operating for the cure of hernia. I take pleasure in emphasizing the tribute which he pays to this distinguished surgeon. The operation, which is called after his name —the open method of the treatment of the wound—is, however, not new. In substance it is described by Heister (1750)—packing the wound by pellets of lint, after the manner of Petit, re-dressing every two or three days with spirits of wine, until the wound is healed, and then applying a truss.

I believe the application of a truss is a mistake, after any of the so-called methods of cure. It causes more or less rapid absorption of the newly proliferated tissue, upon the continuance and consolidation of which the permanency of the cure, in a large measure, depends. This is especially true when pressure

[1] The Anatomy and Surgical Treatment of Hernia. D. Appleton & Co., New York, 1892.

has been applied over the exceptional amount of newly-formed tissues about the site of buried animal sutures.

After the hernia has reappeared the application of instrumental supports may be discussed; never before.

It is now too late to occupy the time of a Society composed of leading surgeons in discussing the question of the curability of hernia. The case is no longer on trial; the evidence is ample, and the verdict has been rendered by a general consensus of opinion by the leading surgeons of the world. As to the better methods, there is still much division of opinion, but the great fundamental factors of the problem are settled, I trust, forever.

In reply to Dr. Gaston's inquiry as to the results obtained by permanent closure of the wound in the cases operated on after strangulation, my experience teaches me that they are usually equally good as when undertaken in reducible cases. It is often easier to satisfactorily deal with the peritoneal sac in strangulated or irreducible cases of hernia.

TREATMENT OF UNUNITED FRACTURES BY RESECTION.

By W. O. Roberts, M.D.,
Louisville, Ky.

A. E., aged twenty-six years, a strong healthy man; family history good; had been a railroader for six years. March 21, 1891, the train on which he was firing being derailed, he sustained a simple transverse fracture of the femur through its middle third. Temporary splints were applied, and he was sent to his home, a distance of three miles, in a spring wagon. Immediately upon his arrival there the entire limb, including the hip, was put in a plaster-of-Paris bandage. At the end of two weeks, the dressing being loose, from subsidence of swelling, was removed and a similar one applied. This was not disturbed for five weeks. Upon its removal there was found free motion at the seat of fracture and but little evidence of callus. There was seemingly no displacement of the fragments and no shortening of the limb. The thigh was now thoroughly rubbed and washed, and a third plaster dressing applied. This was put on over a flannel bandage only. The patient's general condition had continued good. He was now given the syrup of the hypophosphites and directed to go about on crutches. During the following four weeks he was out of doors much of the time. At the end of the four weeks the limb was again examined and no improvement found. It was now put up in plaster for the fourth time, after having been thoroughly massaged, and the patient was directed to bear weight on the limb while using his crutches. At the end of another four weeks it was again examined, with a negative result. I now ordered for him a Smith's brace, which he got in five days. While it was being made the limb was well rubbed daily. Within a short time after the brace was put on he dis-

carded one of his crutches for a cane. The brace was removed in six weeks, and little or no change could be detected at the seat of fracture. The fragments were in apposition, and the shortening was not more than one-half inch. Dr. Vance now saw the case with me. Under chloroform, by forcible movements, we broke up the fibrous union and rubbed the fragments well together. The brace was then reapplied, and in a few days the patient was again on his crutches. He soon discovered that the limb was getting shorter. Four weeks after the operation the limb was examined and found to be one inch short. There was no evidence now of the presence of callus. I advised a resection, but he decided to wait awhile longer. In February he was presented before the Louisville Surgical Society. The only change noticed in the limb was that it was one inch shorter that when last examined. He had worn the brace continuously, and he was then able to walk across the room with the aid of a cane simply. He used one crutch when on the street. Owing to his ability to get about so well with the brace, some of the gentlemen present advised against further operative interference.

March 8, 1892, nearly one year after the fracture occurred, I did a resection, Drs. Vance, Dugan, Pearce, and Henser assisting. A free incision was made on the outer side of the thigh. The fragments were found overlapping about two inches. There was not the slightest sign of union. Half an inch of bone was removed from the end of each fragment with a saw, and an attempt made to approximate them, but one-quarter inch more had to be taken from the lower fragment before this could be done. A hole was now drilled in each fragment, through which they were tied together with a few strands of silkworm-gut. The limb was then put in a plaster-of-Paris dressing. We endeavored to make the operation antiseptic. An abundance of iodoform and bichloride gauze was used in the dressing. On the day following his temperature was 101° F., but soon came down to normal. The pulse ranged from 80 to 90. On the tenth day the temperature reached 102° F. and the pulse 108. That night he had a sweat. On the morning of the eleventh day the temperature was 102° and the pulse 112. On removal of the dressing pus welled out of the wound. There was very little swelling of the thigh and no discoloration. The wound

was now thoroughly irrigated with the peroxide of hydrogen, and a fresh dressing applied. The following morning a window was cut, so that the wound could be properly dressed. The temperature was now normal, and continued so. Irrigation with the peroxide of hydrogen was continued at intervals until suppuration ceased. At the end of nine weeks the wound had healed to a small fistulous tract and good union of the fracture had taken place. The plaster dressing, however, was kept on for a couple of weeks longer. The fistulous tract remained opened until September 21st, when the silkworm-gut came away. The latter part of August several small fragments of bone came away. The limb now is as strong as the opposite one.

The treatment of ununited fracture by resection is more than a hundred years old, White, of Manchester, having done the first operation in 1760. In consequence of the great mortality attending the operation, it was abandoned until revived by Sir Benjamin Brodie. In 1805, Horeau, after having divided the fragments obliquely, fastened them together by carrying a metallic wire around them; Rogers, of New York, in 1838, passed the wire through holes drilled in the wall of the fragments, and then twisted it. Since then other surgeons have used sutures of various materials in the same way, some of them leaving the sutures in permanently, while others remove them after union of the fragments has occurred. Some, instead of drilling the bone, pass the sutures simply through the periosteum. Screws, nails, ivory pegs, and clamps have been used for the same purpose. Roux and Hamilton have tried wedging one fragment into the other. Bigelow and his followers attach great importance to dissecting up the periosteum, so as to preserve it, before removing the ends of the fragments. He reported in 1868 eleven cases treated in this way, with only one failure. Macewen and others attach no importance to the preservation of the periosteum. Nussbaum, in 1875, operated by transplantation in a case of fracture of the ulna with loss of substance. Cutting a portion of bone from the upper fragment, and without detaching the fibrous attachments, he brought it down so as to

fill up the gap. Since then bone from the lower animals has been successfully used to fill up the gap where there is loss of structure. Mr. John Chiene, in cases of loss of substance in fracture of the radius or ulna, removes a section from the opposite bone.

In the long bones, when coaptation of the fragments can be secured, I feel satisfied that resection and a fixed dressing will be followed by just as good results as when sutures or other contrivances for fastening the ends of the fragments together are used. In the *Annals of Surgery* for this month there appears an abstract of a paper by Dr. Hennequin, of Paris, containing reports of five cases of pseudo-arthritis—four of the humerus and one of the thigh—in each of which two operations of resection and wiring the fragments together were done, all of which were unsuccessful. In his comments, Dr. H. contends that "when the fragments are in contact, without interposition of foreign tissue, or when the interval separating them is slight, the suture is without influence in the formation of the osseous callus, which will form, when the constitutional conditions of the patient are favorable, as well without as with the suture, but which will be wanting notwithstanding the refreshing and suture of the fragments, if the system is under the influence of one of those tendencies, the nature of which we are still ignorant of, which renders it powerless to effect a bony union."

Treves states that he used the metallic suture at one time, but had good reasons to give it up, and that he has had far better results since abandoning it.

As to the results of resection in the treatment of pseudo-arthritis Norris's table, which contains 150 cases, shows 113 cures and 6 deaths. All of the fatal results were in cases of fractures of the femur.

Agnew's list contains 178 cases, with 104 cures and 11 deaths; of the 11 fatal cases 8 of them were pseudo-arthritis of the femur.

FRACTURES OF THE VAULT OF THE SKULL, AND THEIR TREATMENT.

By W. C. Dugan,
Louisville, Ky.

Injuries of the skull have been recognized and active steps taken to relieve them, even in the days of prehistoric medicine. Ample proof of this is found in old skulls, presenting unmistakable signs of trephine operations.

In the last decade invaluable work has been accomplished in cerebral topography, by which the surgeon is enabled, almost with mathematical precision, to excise irritable cortical centres, to aspirate the dropsical ventricles, remove cerebral neoplasms, etc. Intractable trifacial neuralgias are being successfully relieved by neurectomy within the skull, and crainectomy, one of the most modern surgical procedures is now on probation, with some prospect of proving a boon to the microcephalic infants. The novelty of these operations very naturally enthuse the surgeon, and the degree of success achieved bespeaks a new era in cerebral surgery.

But, instead of inviting your attention to the consideration of one of these fascinating subjects, I shall briefly discuss one more common—an every-day subject, as it were—yet one which concerns a greater number and to a greater degree, "Fractures of the Vault of the Skull, and Treatment."

When graduated I was so familiar with the tables of differential diagnosis of compression and concussion, as found in text-books, that I would have felt a reproach on my professional accomplishments had it been thrust at me, had it been intimated that I might experience some difficulty in making

a diagnosis of fractures of the skull, with depression from rational symptoms. But, after some years of experience, I am ready to join one of the latest authors on the subject in saying that "Fractures of the calvarium, whether of the vault or base, show no special rational symptoms that may not arise from cerebral contusion, laceration or hemorrhagic, without fracture."

If with each fracture of the skull, with depression, there developed symptoms of compression, such as stertorous respiration, paralysis of the special sensory, sensory and motor nerves, the diagnosis would be on the surface, and the treatment very clear indeed.

But every surgeon who has long been engaged in practice fully realizes the lack of dependence to be placed in the sharp, differential distinctions, and further, that the so-called rational symptoms of depression of fracture are often absent for an interval of days and even years.

I regret to say there is a strong disposition to disregard all fractures of the skull surgically when there are no so-called typical rational symptoms, which I hope, in this short paper, to prove is dangerous, false conservatism, and is accountable for many cases of epilepsy and insanity.

There is a belief, and, too, a pretty general one, even among physicians, that the cranial cavity is sacred to the encephalon, and will admit of no encroachment on its contents without an immediate manifestation of symptoms of compression. While, in fact, Nature is so well provided for against compression by her numerous ventricles and subdural space, she will tolerate for a variable period of time considerable pressure.

In dealing with fractures in the adult and child we must must bear in mind the marked anatomical difference in the skull of the young and old. The skull of the child is thin and vascular, and not susceptible of being divided into tables, while that of the adult is thick, strong, and elastic, and composed of two tables and a vascular zone. The external table is much thicker than the internal, and gives the strength and

elasticity to the skull, while the internal is thin, inelastic, and very easily broken. Fractures in the adult may involve the entire thickness of the skull or be limited to either table. With a correct knowledge of the external table we can readily understand the improbability of its being fractured alone. It occasionally occurs, however; but as a rule, the lesion is either over the frontal or mastoid region. Fracture of the internal table alone is a possibility, and I am inclined to believe that it occurs more frequently than is generally thought.

Here the fracturing force, not being sufficient to entirely overcome the elasticity of the external table, which, after being forced in, rebounds to its normal position, leaves the internal table fractured and not infrequently depressed. For this same reason we find the extent of fracture, when both tables are involved, far greater in the internal than the external.

The character and extent of fracture as seen in the external table is no criterion by which to judge the extent of the lesion in the internal; nor can the absence of fracture in the external table enable us to exclude fracture of the internal. So, in dealing with fractures of the skull, we may lay it down as a rule, to which there are but few exceptions, that when a fracture of the external table exists, it matters not how insignificant it may be, there is also one of the internal. For convenience, we may divide fractures, here as elsewhere, into simple, compound, and complicated.

It would be well to keep in mind the fact that extensive fractures of the skull, with complications, may exist without scalp wound. In such cases, and these are common, it is difficult, and at times quite impossible, to make a diagnosis, even with extensive depression, without incising the scalp, owing to the early formation of hæmatoma. The symptoms of fracture of the skull without concomitant injury to the cerebral tissue are not by any means clearly defined by disturbances of the functions of the nervous system, and for that reason alone, both compound and simple fractures are overlooked

till meningitis or epilepsy develop. The first point, then, which I desire to emphasize is that the symptoms of the non-complicated, depressed fractures, to be found at the time of the accident, are not those of compression. In fact, the only symptom to which any importance can be attached (barring shock) is the existence of depressed bone, detected either by the finger or probe, or the line of fracture exposed so as to be seen. These patients, as a rule, are rendered unconscious for a very short time; then reaction comes, and they get up and walk away, or even resume their work. If they should find a scalp-wound they call to see a physician to have some "sticking plaster" applied, or, if there is simply a swelling, to secure a "liniment," little suspecting the serious character of the injury.

It is very common for physicians to close scalp wounds over extensive fractures of the skull, and give a favorable prognosis, and are not a little surprised to be called back soon to treat his patient for menengitis, or later for epilepsy. In complicated fractures, either simple or compound, with laceration and contusion of cerebral tissue, the symptoms of compression are pronounced from the very beginning—the essential lesion being the damage sustained by the brain. Symptoms of compression coming on sometime after the injury are caused by the depressed bone exciting certain vascular changes in the cranial contents. So thoroughly am I convinced of this, that when I see a patient presenting symptoms of compression soon after injury of the head I give a doubtful prognosis, and further, do not expect to see the symptoms entirely relieved by the elevation of depressed bone, if such should exist. Fracture of the skull in itself is not a serious injury. The danger to apprehend is as follows:

1. The damage sustained by the brain and its membrane.
2. The delay of surgical interference.

My experience with fractures of the vault of the skull has been such as to justify me in the two following general conclusions:

1. The rational symptoms of compression coming on im-

mediately after the receipt of an injury are due to injury of the cerebral tissue.

2. Those coming on later, hours or days, are due to congestion or inflammation of the cranial contents, excited by irritation of the depressed bone. In order to emphasize this point and to further sustain the position I have taken concerning the symptoms of fracture, I shall ask the privilege of briefly reporting several cases.

Case I.—Mr. F., aged twenty-two years, came into the hospital, with history of having been struck on the head with a brick. He walked into the hospital, feeling that his condition was not at all serious. When examined, it was found he had an extensive scalp wound in the centre of his forehead. On closer investigation, it was ascertained that he had a depressed fracture. He was anæsthetized, scalp cleansed, and wound enlarged, and an area of depressed bone, two by three inches, brought to view. The bone was detached and driven down on the membranes. The depressed fragment was elevated, and patient made an uninterrupted recovery.

Case II.—Mr. T., aged twenty-five years, with compound depressed fracture, without symptoms. He was brought to the hospital and give the history of having been struck on the head with a hammer. When his head was examined it was found that he had a depressed fracture. He was anæsthetized and the wound enlarged, and a depression the size of the pole of an ordinary hammer, located over the motor area of the left side, was exposed. The depressed external table, along with a much larger area of the internal, was removed. The patient made an uneventful recovery

Case III.—Patient with compoud, depressed fracture, without rational symptoms. He walked to the college clinic to have what he thought was a simple flesh wound of the head dressed. But when examined it was found he had fracture of the skull. He was anæsthetized, wound enlarged, and depressed bone elevated. After the operation he insisted upon walking home, several squares, and he returned to the clinic several times to have his head dressed, and was then discharged cured.

Case IV.—Patient with compound, depressed fracture, came to

the clinic to have head dressed. On examination, fracture was diagnosed. Patient refused to submit to operation and left the clinic, and has since become an epileptic. Now, notwithstanding the fact that each of these cases had extensive depression, not one of them presented any of the rational symptoms of fracture.

Case V.—Child, aged five years, simple fracture with great depression, without symptoms. Patient had fallen into an empty cistern, twelve feet deep. Patient had considerable shock, but when I saw it in consultation it had reacted. Owing to the large hæmatoma we were unable to make a satisfactory examination, but considering the distance fallen, fracture was expected, and an incision of the scalp advised, and consented to. When the scalp was turned back an extensive fracture, extending from the posterior to the anterior border of the pariental bone, was found, the lower part being forced under the upper. The overriding bone was removed with the mallet and chisel and the depressed bone elevated. Child recovered, and was out in a very few days.

In this connection, at the risk of becoming tedious, I wish to report several cases where the symptoms of depression came on at the time of the injury and were not relieved by the removal of the depressed bone.

Allen C., aged thirty two years, received two severe blows on the left side of the head, with a dirt shovel, resulting in extensive scalp wound and fracture of the skull, with depression. Symptoms of compression came on at once. The depressed bone was elevated within one hour, but with no perceptible improvement, patient dying at the end of one week. Post-mortem revealed great damage to the brain, which was the cause of death.

The next two cases are so near alike that we will report them together. They were both brought to the city hospital with gunshot fracture of the skull, presenting all the symptoms of compression. The fragments of bone and blood-clot were removed, the ball having passed through and out of harm's way, but the patients made no improvement. Post-mortem showed extensive brain lesion. These three patients all had symptoms of compression which were not relieved in the slightest by the removal of

depressed bone, showing that the symptoms were due to brain injury and not to pressure.

TREATMENT.—Contrary to the teaching of some of our very best text-books, I am opposed to the so-called expectant treatment. The antiphlogistics and sedatives should be superseded in fractures of the skull, as they have been in pelvic inflammations, by definite surgical interference. The idea of waiting for the development of rational symptoms of compression before instituting measures to relieve depressed bone is as bad surgery as waiting for stercoraceous vomiting and general peritonitis in strangulated hernia before operating.

When we know that the mortality of the operation to elevate depressed bone is but a little above one per cent. before vascular changes are excited, whereas, if the operation be put off till the symptoms of compression develop, the mortality is over thirty-five per cent., it does not seem that stronger argument in favor of earlier and more universal surgical interference could be produced.

The danger of operations on the skull is far less than is generally believed. Modern surgical technique has rendered trephining so safe that they are classed along with amputatations of the last phalanx in point of danger. In elevating depressed bone it is generally found necessary to fenestrate the skull, which may be done either with the chisel or trephine. I am rather partial to the chisel and mallet. It is a rapid method of operating, and exact, and if the chisel be held so that it makes, with the tangent of the circle at the point of operation, an angle of 15° to 20,° the shock that would otherwise be produced by the stroke of the mallet is practically done away with. Instead of asking ourselves what class of cases should be operated upon, the question *is*, what cases should *not* be operated upon. This important question comes up with each case, and the future happiness and usefulness of the individual depend much upon our decision.

1. I would say all cases of punctured and gunshot fractures, with or without symptoms, should be explored without delay.

2. All compound fractures, regardless of symptoms, are cases for exploratory operations. Palliative and expectant treatment should be regarded as inefficient and hazardous. The only exception to this rule is in children, where there exists neither symptoms nor depression.

3. All simple fractures with depression, regardless of age, with or without symptoms, are not to be temporized with, and the patient's condition regarded as serious till every part of the depressed bone is removed or elevated.

4. All simple fractures with symptoms should have the scalp incised and the skull trephined, for no one is able to predict the condition of the inner table by the character and extent of lesion found in the external.

5. All cases with symptoms of compression, without the existence of fracture of the outer table, should be subject to an exploratory craniotomy, provided the symptoms are such as to enable the operator to be reasonably certain of the location of the lesion.

6. I desire to place myself on record as favoring exploratory operations in all simple fractures in the adult, even in the absence of symptoms of depression.

So thoroughly am I convinced of the wisdom of exploratory craniotomy—of our utter inability to judge the condition of the deeper parts of the fracture by that which we see on the surface—and the great danger attending all cases where the fragments of the internal table are forced down on the brain, of the ease and safety with which these operations are done, that if I were the victim of such an injury I would request that the skull be opened.

The failure to diagnose fractures of the skull is one of the blunders of hurried and incomplete examinations. A patient presenting a scalp wound or contusion, with a history of having received a severe blow, kick, or a fall on the head, should be looked upon with suspicion, and the most thorough examination instituted for fracture. If it be ascertained that it is impracticable to make a satisfactory examination without, anesthetize the patient, for it is all-important that a correct

conclusion be reached. No one should close a wound of the scalp till he is able to say positively that no fracture exists. If there remain the shadow of a doubt as to the existence or non-existence of fracture, that doubt should be interpreted to mean that the scalp must be incised and the bone examined. In the doubt lies the danger!

DISCUSSION OF THE PAPERS OF DRS. ROBERTS AND DUGAN.

Dr. Bedford Brown, of Alexandria, Va.—I have seen some quite remarkable cases of fracture of the skull with recovery. In 1860 a case came under my care of fracture of the frontal bone of the skull in a boy sixteen or seventeen years of age, produced by the kick of a horse. The frontal bone was fractured three inches across and an inch and a half in width. It was a compound comminuted fracture. Portions of bone were driven back into the frontal lobes nearly an inch, yet the boy was perfectly conscious, and had not the least indication of symptoms of compression of the brain.

Dr. Gaston.—Will you define more exactly the location of the fracture?

Dr. Brown (resuming).—It was across the frontal bone, extending three inches and an inch and a half in width, corresponding to the size of a large horse's hoof. The boy was so intractable and ungovernable that I determined to put him under an anæsthetic. At that time the use of anæsthetics in the treatment of injuries of the brain was an experiment. We had never heard of it, and it was a serious matter to consider, so much so that we debated for some time whether we should use pure chloroform or ether, or both combined. Finally, I concluded to combine the two, hoping the ether might in some way overcome the depressing effects of the chloroform. The boy's pulse and general condition were good; he lost a large amount of blood. He was placed under the anæsthetic, and we removed all the spiculæ of bone driven into the brain. We used the trephine only in one place. When we removed the lacerated portions of the brain and bones there was an immense cavity in the frontal lobes of the brain. This is the first case on record

in which the action of chloroform was demonstrated upon the physical condition of the brain. The case was published in the *American Journal of the Medical Sciences* more than thirty years ago, and was copied extensively in other journals. The boy was put under the influence of chloroform during that protracted operation five times. Each time, when the effects of the chloroform passed away, a hemorrhage occurred, the brain became turgid with blood, congested, red, and commenced pulsating. Just as soon as he got under the influence of the anæsthetic all these symptoms disappeared, the brain became anæmic, the hemorrhage ceased, the turgidity all disappeared, and the pulsations were quieted. This thing was demonstrated five different times during the operation. After all the loose spiculæ of bone were removed and the loose brain substance, the wound was dressed. The boy recovered consciousness ; in a week's time the pulsation of the brain cut through the silk sutures—we had no silver wire in those times. The fungus cerebri increased in size until it was as large as a hen's egg. I dressed it according to the method of my preceptor, Dr. Dudley. I first placed a thin piece of linen over the fungus, then over that a sponge, just enough to be firm ; the secretions gradually caused the sponge to expand, and, acting as a graduated compress, forced the fungus back within the skull gradually. The wound healed kindly. I saw the boy after I returned from the war, and the opening in the frontal bone was such that you could see every pulsation of the brain, yet his mind was bright. He was then a grown man. He suffered pain occasionally, but with that exception he had no difficulty, either mentally or physically.

The next case came under my care in 1862. A man was brought into my field hospital with a grape-shot imbedded in the centre of the frontal bone. It had gone in between the two lobes of the brain a considerable distance. The ball was wedged in so tightly that we had to saw the edges around in order to extract it. The man was perfectly conscious and requested us not to place him under the influence of an anæsthetic. He was a soldier. Well, we finally removed that bone and the loose portions of brain that were lacerated, dressed the wound—but not antiseptically, for we knew nothing of antisepsis in those times—with boiled water reduced to a proper temperature. He

continued in my field hospital for some time. There was a small fungus which protruded in the case, which was treated after the same method. The wound finally healed, the man had no mental disturbance and scarcely any fever.

Dr. J. H. Letcher, of Henderson, Ky.—While I indorse what Dr. Dugan has said, with the average country practitioner it is a serious matter to open the cranial cavity. I see very readily how, with expert assistants, it can be done without apprehension of trouble. In emergency cases you cannot afford to send them to a surgeon for operation, and oftentimes when you think there is depression, bad results do not always follow. I desire to cite a case in point: Last year Dr. Lewis, in my part of the State, called me in to see a case in which there was marked depression of the skull. When the boy was coming out of a coal pit a hammer, weighing eight or ten pounds, fell on him. His pulse was 55 for three or four days. He would not submit to an operation, and finally got well. I think it is well enough to bear in mind that trephining should be done in a great many of these cases, but we should not be too hasty in resorting to the trephine and chisel.

Four weeks ago I saw a case with Dr. Stewart, of Zion, the patient being kicked in the head, and as a result a good deal of brain matter was lost. The wound was nicely dressed and the man was doing well. I mention the case to show what the brain can stand, and to further say that it is well to be careful in the use of the trephine or chisel, not doubting that we are all careful in these cases. When country practitioners come across these cases they cannot readily take them to expert operators.

Dr. Cornelius Kollock, of Cheraw, S. C.—I have trephined the skull four times in cases of fracture. Three of the cases were boys, and one boy was not more than five years old. He was kicked about ten feet by a mule. The skull was driven in and fractured so badly that one portion of it an inch long was detached from the other portions. I removed that piece, and the boy recovered. Another boy was kicked by a horse, and his case was similar to the first. The third was thrown from a horse and fell against a tree, fracturing his skull. He was insensible. I trephined, and he immediately became conscious. The other case is a little more interesting. A young man was out gunning

with a friend, and while seated on a log he said: "There is a squirrel! Shoot him." He raised his gun and fired. The gun burst and struck his friend and fractured the skull. He finally recovered, but had epileptic fits for two years. He could not take a glass of beer or wine but what it would bring on epileptic fits. The fits occurred so frequently that they began to affect his mind a little. The patient then concluded to have an operation done if it killed him. He was brought fifty miles to me. I trephined, found a spicula of bone pressing upon the brain, and the least excitement distended the brain till it would press up against the piece of bone, and the patient would have an epileptic paroxysm immediately. I removed the spicula of bone, closed the wound, and watched the patient closely for a week. On the evening of the operation, at 6 o'clock, he had an epileptic fit, and it was the only one he has had since. It is more than ten years since the operation was done, and the young man seems to be in perfect health; does not have the violent headaches with which he suffered previous to the trephining, and his mind seems to have regained its equilibrium.

Dr. J. H. McIntyre, of St. Louis.—It affords me great pleasure to take part in the discussion of this interesting subject. I used to be very much interested in the operation of trephining. It has fallen to my lot to do a large number of operations for trephining the cranium, more, however, in years gone by than in the more recent ones, because as a great many of you know I have drifted into a specialty which took me out of this class of work.

I heartily indorse the position taken by the essayist in his paper, and with the permission of the Association I will cite a case or two bearing on the subject. During the reading of the paper I was reminded of the case of a German who had been out from Germany but four or five months; he could not speak a word of English, and in company with an Irishman he was set to work on a farm at cutting down trees. They had nearly cut the tree down, and when the tree began to crack the Dutchman ran; after running a short distance he turned to see how the tree was getting along, and in falling it struck him on the anterior part of the head without any break upon the skin, and he immediately fell to the earth insensible. I saw him thirty-four hours

later. He was in profound stupor, and had been since the time of the accident. I found him with slow stertorous breathing, respiration not more than four or five in a minute, and with a pulse of less than 60. The result of the falling of the tree was disclosed upon laying open the scalp freely. Extensive scalp dissection was required to trace the direction of the fracture; when a triangular piece of bone was removed and the depression taken off he began to swear in Dutch, and started in where he left off in cutting down the tree.

Another case of some interest in the way of trephining for traumatic insanity occurred in a railway employé. Several weeks after the receipt of the accident, wherein there was extensive fracture in the upper Rolandic region, the patient developed homicidal and suicidal tendencies of marked degree, so much so as to require the constant attention of a nurse by day and another one by night. For the space of eight or ten days, before my seeing him, his doctor thought of sending him to a lunatic asylum. This happened upon the St. Louis and St. Francisco Railroad at the time I was managing the surgery of it. There was no break upon the skin in the locality of the fracture. He was in an insensible condition for five or six days after the receipt of the injury.

Another interesting case, a German, six feet one in his stockings, twenty-two years of age, weight 200 pounds up to the time of his coming under my care, sustained a fracture of the skull by being thrown violently against a tree. A pair of mules ran away with him in a wagon. He was not operated on at the time, and received only external applications. The fracture was not recognized. There was a slight scalp wound over the site of the fracture. Three months afterward he developed epilepsy. The epileptic paroxysms continued for two years after the receipt of the accident. Suffice it to say, I subjected him to trephining for the relief of epilepsy. The injury was in the region I will indicate, and over the sinus a large trephine was applied. When it was removed there was an exostosis, with its grasp protruding from the button of bone, and with it the button of bone came out in the trephine without any difficulty. I do not believe that I wounded the membrane of the brain with the trephine. I am satisfied I did not, but a thinning had occurred on account

of pressure occasioned by the exostosis, and when the pressure of the osseous tissue was taken off a stream of blood burst through, and there was a hemorrhage such as I have never seen before in all my experience—a hemorrhage which had a tendency to produce a diaphoretic effect on a man. Suffice it to say, compression was resorted to with gauze and sponges, and it did not control the hemorrhage. I began to have serious fears for the man's life, thinking possibly he would die upon the table. But by the grace of God or something else, I happened to have in my bag a large cork, possibly a little larger than a No. 7 or a No. 8. A bright idea struck me. This cork was shaved with a scalpel and the end dipped into an antiseptic solution of 1 : 1000 bichloride of mercury and washed off. I twisted it into the wound just as I would put a cork into a bottle, being careful to cut the cork off so that it would not press upon the membranes of the brain and only take the depth of the bony structure. That stopped the hemorrhage at once. After my experience with that case, and whenever I am expected to do a trephine operation, I always carry two or three big corks in my supply bag.

Dr. William Warren Potter, of Buffalo.—One of the very interesting portions of the many-sided question that Dr. Dugan has so intelligently presented is that relating to fracture of the internal table of the skull without a corresponding lesion of the external table, and it is to that portion of the paper that I wish to speak. There is a great liability to err in diagnosis when the surgeon cannot detect a fissure in the outer table at his examination after traumatism of the head. He is apt to conclude that there has been no bone injury, and that his patient is simply suffering from concussion. It is understood, of course, that I am speaking of those surgical injuries of the head which are followed by symptoms.

It is interesting in connection with this subject to recall the studies made by Mr. Tevan relating thereto. In the *British and Foreign Medico-Chirurgical Review* for July, 1865, these investigations that I refer to were published. Mr. Tevan was led to investigate the subject from the fact that fracture of the internal table without external fracture was denied by some, doubted by many, and admitted by few. His experiments may be summarized as follows: He first stripped a skull-cap of its soft parts,

put a wet cloth inside of it, and laid it with its convexity in the palm of the left hand, which latter he protected with several layers of moist cloths to prevent or obviate an inconvenient amount of pain. He then struck the inside of the calvarium with a hammer with only slight force, which produced a fracture of the external table without any fracture of the internal. It will thus be seen, therefore, that whether the inside or outside be struck, fracture of the distal table only occurs, and that without injury to the proximal table. A like analogous condition may be produced by bending a stick across the knee, when it will be observed that fracture begins not where the force is applied, but on the opposite or distal side of the stick, which is in obedience to the physical law that fracture commences in the line of extension and not in that of compression. The lessons to be learned from this series of experiments, as well as from the clinical cases that bear them out, is that great caution must be observed in making diagnosis and prognosis in regard to all head injuries.

I have seen at least one case which corresponds to the teaching of Mr. Tevan, in which there could be discovered no fissure of the outer table, and it was supposed to be only a scalp wound. The boy, a lad about ten or twelve years of age, was hit in the morning with a stone thrown by the hands of a playmate, and after receiving some slight attention was permitted to go to school. While in school he became comatose and was taken home. At the operation it was discovered that there was a fracture of the internal table which crossed two arterial filaments that caused bleeding. A six-ounce clot lay underneath the button of bone that was removed, and there was no detectable fissure of the outer table.

I might speak of some other interesting cases upon which I have operated, but my purpose in these remarks was to direct attention to a single point in Dr. Dugan's valuable paper, namely, the importance of the close study of all head injuries, with reference to a possible fracture of the internal table of the skull without any discoverable lesion of its external table.

Dr. Howard A. Kelly.—I wish to record in this connection an interesting case of fracture of the base of the skull, occurring during my residence in the Episcopal Hospital, in

Philadelphia. A man of about sixty years was brought into the surgical ward in a comatose state, and the only history we had was that he had fallen on the ice and struck the back of his head. He had all the symptoms of a fracture of the base of the skull, and in examining his urine I discovered that he had marked glycosuria; the natural conclusion was that this was one of those interesting cases of fracture of the base of the skull causing glycosuria. A more thorough examination, however, revealed the fact that he had a diabetic cataract, the diabetes was, therefore, simply an accidental pre-existing condition complicating the fracture.

Dr. James Evans, of Florence, S. C.—I have had two cases of penetrating gunshot wound of the cranium in boys which are interesting on account of their perfect recovery, notwithstanding the balls have never been removed from their brains, showing that it is not always necessary to trephine or make a fruitless search for them.

B., aged fifteen years, in September, 1886, while handling a cocked and loaded pistol, endeavored to look down the barrel, the pistol was discharged, the ball, the size of an ordinary buckshot, penetrating the forehead half a line to the right of the centre and one inch above a line joining the eyebrows. At the discharge of the pistol, he fell to the ground and was taken up apparently lifeless, and brought to a house not many yards distant from the scene of the accident, and where by chance I was on a visit at the time. The shock was profound, countenance pale, scarcely any pulse, and the wound bleeding freely. As soon as reaction took place, consciousness returned, his intellect became clear, he answered questions intelligently and complained of dull, aching pain in the head. His expression was natural aside from indicating pain, no deviation of tongue, and apparently very little impairment of muscular action, as with an effort he could perform any movement required. While lying on his back a probe inserted in the wound took a direction downward and backward, and entered without any force, being brought to bear on it two and a half inches. The probe being pushed on in the same direction would have impinged on the vault of the cranium just posterior to the coronal suture. Not being able to locate the position of the ball, no attempt was made to extract it.

All spiculæ of bone were removed, and the wound having been thoroughly cleansed with a solution of permanganate of potash, was closed with a pledget of carbolized cotton held in place with a bandage. On the fourth day there was high fever attended by active delirium. The dressing of the wound was removed and a drachm of serum was discharged, which at once quieted the delirium and relieved the pain in the head almost entirely. A decided paralysis of right lower extremity was now observed. The next day the right upper extremity became similarly involved. The temperature became normal on the twelfth day, and on removing the dressing from the wound it was found healed. The paralysis gradually disappeared, and the boy has grown to be a vigorous and healthy man, and exhibits no trace of the former injury to his brain. The other boy was eight years old at the time of the accident, which occurred sixteen years ago. He was shot with a 22 calibre pistol, the ball entering an inch and a quarter above the right eyebrow and half an inch from the middle line. The ball could not be found or located. The paralysis of the left half of the body was more marked in this case, but he fully recovered the use and strength of his limbs, and I saw him plowing in a field a few days ago.

Dr. William T. Briggs, of Nashville.—Mr. President: I was not present when the paper under discussion was read, but from the remarks of the gentleman who has just taken his seat I infer that trephining for the relief of epilepsy resulting from injuries of the cranial bones was the subject of discussion.

It has been my fortune to have trephined more than fifty patients suffering from epilepsy, the result of injury of the cranial bones. A large proportion of the cases (fully four-fifths of the number) have been followed up and closely watched for several years. Nearly every one subjected to the operation has been permanently cured. The changes in the condition of the bone as revealed by the trephine have been different in the different cases. In some long and sharp osteophytes; in others, a boss or nodule have formed. In others still an irregular union of the fracture had taken place. Again, the bones have been found in a sclerosed condition with complete obliteration of the diploic structure. In a few cases a careful examination of the buttons of bone has revealed no change whatever in structure or contor-

mation. I have been very careful to select the cases I subject to operation, and unless a localized point of irritation is induced by pain, tenderness, or other symptoms is present, I decline operative interference. The success in these cases depends in my judgment upon (1) the removal of all of the bone which is acting as an irritant; (2) in the proper treatment of the wound after operation; (3) in the dietetic, hygienic, and medicinal treatment of the patient for several years after recovery from operation.

The President.—Have any of the patients died as the result of the operation?

Dr. Briggs (resuming).—No. One of my cases died after the operation. The patient lived fifty miles from my home, and was left in charge of his physician with instructions to leave the wound open and well-drained. On the second day after the operation the wound was closed with adhesive plasters. Coma ensued, and I was hastily summoned by telegram. As soon as I reached the patient the plasters were removed, and the wound freely opened. Nearly half a pint of bloody serum escaped, but the damage had been done. My patient died not from the operation, but from improper after-treatment.

Dr. McGuire.—Did you operate antiseptically?

Dr. Briggs.—Yes. After thorough aseptic preparation of the parts to be invaded I treated the wound by the open method, and irrigated its whole surface with hot water every few hours.

Dr. Thad. A. Reamy, of Cincinnati.—As pertinent to this discussion I desire to refer to a case which, so far as I know, has never been published. In the winter of 1859–60 I was teaching in the Cincinnati College of Medicine and Surgery. A colored subject was brought into the dissecting-room. The man had died of smallpox before the eruption had advanced sufficiently for diagnosis by those who were to use the material. As a result many cases of varioloid and a few cases of variola appeared in the class of students. This man's skull had been fractured and depressed. The fracture started near the posterior limit of the saggital suture, and extended forward parallel with that suture but one-fourth of an inch to the right of it for a distance of two and one-half inches. The bone depression along the suture border of the fracture was at least one-third of an inch in the centre, but less at the ends. Some absorption of the depressed border had taken place.

The history of this man was procured and read to the class. Some six years previously he had fallen from the second story of a pork house, having at the time a dressed hog on his shoulder. In the fall his head struck upon the corner of the wagon tire below. He manifested signs of compression for twelve hours after the accident. He was trephined, but the depressed bone was not elevated. He made a speedy recovery. Never suffered of epilepsy, headache, or other nervous symptoms, dying as above indicated, six years later, of smallpox. The specimen was placed in the College museum, where I suppose it now is.

Dr. G. Frank Lydston, of Chicago.—I wish to corroborate what Dr. Dugan has said regarding symptoms of compression often being due not to the injury of the bone or depression *per se*, but to secondary changes coming on as a consequence of the injury. We very often have a condition to which Dr. Briggs alluded in a cursory manner, that explains some of the obscure cases of epilepsy following remote injury to the skull, and explains moreover, those intractable forms of headache following injuries of the skull, viz., sclerosis of the bone tissue proper. It is a fact—and I think most operating surgeons are perfectly familiar with it—that in certain instances of epilepsy following traumatism, at a remote period, trephining is followed by a perfect cure, although no change in the membranes was detectable at the time of operation, nothing being found excepting the sclerosis of the skull proper. Why this occurs there is no satisfactory explanation. In some of these cases there are no immediate symptoms of profound injury, and yet they are followed at a remote period by severe headache and epilepsy. Dr. McIntyre brought up an excellent point in his description of a case in which there was exostosis. He said he did not injure the meninges in removing the button of bone. I do not think he could have removed that button of bone without injuring the meninges by virtue of certain pathological conditions that exist in those cases. In those cases in which an injury of the internal table of the skull is followed by exostosis there is always a pachymeningitis externa, a consequent condensation of the meninges at that point, and adherence to the bone. I do not think it is possible in cases of that kind to remove a button of bone without injuring the meninges. They would be necessarily torn through.

One point I think is illustrated very nicely in cases of fracture of the skull, namely, that they bear a very close similarity to certain cases of penetrating wounds of the abdomen. It is generally understood by all practical surgeons that an exploratory operation in cases of penetrating wounds of the abdomen does not complicate matters. The point cannot be too strongly insisted upon that the same is true of injuries of the skull. If there is a depression of the skull it is *prima facie* evidence that the membranes, meninges and brain itself have been injured, and certainly an operation does not complicate matters, and removes the conditions which must have been produced at the time of injury. The symptoms of compression and other conditions may be produced later on. In these days of aseptic surgery a clean operation does not complicate matters in cases of compound fracture of the skull, and it seems to me that in every instance of this kind it is the surgeon's duty to do an operation and remove any depressed fragments of bone that may be present, and if no other result ensues from the operation it will at least enable us to explore for the purpose of determining the amount of injury present.

Dr. William Perrin Nicolson, of Atlanta.—I was not present when Dr. Dugan's paper was read, and I understand the paper of Dr. Roberts upon the subject of "Ununited Fractures" is under discussion with it. I know of nothing that concerns the general surgeon more seriously than the subject of fractures, and I have had some experience with ununited fractures occurring from different causes. In one of the cases there was fracture of the femur, which was treated by another surgeon, the case coming under my charge about the third month after the injury. It finally yielded after I put a plaster dressing from the knee up around the waist and compelled the man to walk upon it. As far as I could tell the case was due to insufficient dressing, because the surgeon only applied the dressing from the knee to the upper part of the femur, and the fracture involved the upper segment.

Two other cases that have come under my care are more interesting. They were cases in which complete re-absorption of union has taken place. One was a patient who had had a severe crushing injury from the wheel of a street car passing over

the forearm, causing compound fracture of both bones. I dressed the injury, and after some weeks the man's forearm was apparently strong, and the union seemed as good as one could desire; I therefore took off the dressing. About one week afterward he came to me and said the line of union was moved, and demonstrated it by placing his hand upon the table, making flexion and extension of his muscles, and I thought I saw a slight motion. He came back at the end of another week; then it was very perceptible. I put it back in dressings, but it gradually gave away until there was nothing but fibrous union of the radius and ulna.

After going around for some time, entirely deprived of the means of making a livelihood, he came to me, and I suggested cutting down upon the fracture and uniting the bones with silver wire.

He consented to the operation, and I made an incision over the ulna and radius, found the radius in good position, but a gap of fibrous union between the fragments. I chiselled and gouged out all the fibrous tissue, drilled the fragments, twisted them firmly together with silver suture, pressing the wire into the line of fracture. I gave my attention to the ulna, and the gap, apparently, being small between the ends, concluded not to apply a silver wire to that, as I thought the wound was dressed antiseptically.

I reported this case once before the Georgia State Medical Association.

The wound suppurated from some cause or other, perhaps someone stuck his finger into it at the time of operation. The fragments were not wired and the ulna did not unite, but the radius united firmly, and that man has for years been pursuing his occupation as a moulder in an iron foundry, in which he has a tremendous amount of work to do with that arm, and it is perfectly strong.

I saw another patient with compound fracture of the humerus, in which after the sixth week I took it out of the dressings, as I thought union was strong. I could detect nothing to the contrary. It gave way in spite of all the treatment I could adopt. Union was apparently absolutely absorbed; Nature came to his relief with a fibrous union, and he has the use of the extremity,

so that he has had nothing done to it. There is a perfectly movable false joint. Such cases are rare; they are referred to in text-books. The union was perfect, and it has been completely and entirely absorbed.

Every surgeon knows that bad results in fractures always reflect upon him, no matter whether he has anything to do with the bad result or not.

During my service in the Grady Hospital for the last few months an interesting case came under my observation. A man came in collision with an electric car and received a number of injuries—fracture of the femur, fracture of the clavicle, dislocation of the shoulder, and a number of other minor injuries. The clavicle united without any trouble. The femur was put up in Buck's extension apparatus with perfect satisfaction, but did not unite. I learned from my successor in the surgical service that the collar bone has united firmly. Whether the failure of the femur to unite was due to something getting between the line of union I do not know. The treatment of ununited fractures I look upon as a matter of great importance. These are the only cases that have come under my personal observation. The case of wiring has been successful. It has been something like four or five years since the operation was done, and the patient has never had any complaint to make from it.

As a general proposition I do not know what can account for the re-absorption of the callus that has been once deposited. We know that there are cases of delayed union in which the bony deposit is of very slow formation, and in some instances it is never formed, but the mysterious forces in nature that are reversed and turned to the destruction of the already formed union must ever be a puzzle to the surgeon. In the two cases that I report here one was a confirmed inebriate, and though it may have accounted for the first failure, after the operation consolidation was perfectly satisfactory. The other was in a man of robust health, and one in every way promising for a good result.

After all, such cases ought to impress upon the surgeon the serious responsibility that accompanies the treatment of fractures, and justify the statement of the great Gross when he said that

to the last day of his life he never approached a fractured bone without fear and trembling as to the result. We should always learn to be careful as to the criticism that we pass upon bad results in the hands of others.

Dr. A. M. Vance, of Louisville.—The subject of fractures of the skull is one of great importance, and one in which not only the people at large, but a good many doctors, need more education in regard to the primary treatment of them. I have operated on a great many cases, both primary and secondary. I have also had a number of cases where the physician in charge and the family would not allow me to operate, and epilepsy has resulted.

I cannot agree with Dr. Letcher that we should delay operative interference in these cases, as I think they should be operated upon as early as possible. I believe Dr. Reamy's cases, 99 out of every 100 of them, will have epilepsy. The fifty cases in which Dr. Briggs trephined show his successful results, which argues against conservatism which results in epilepsy. I fully indorse what Dr. Dugan has said, that it is better to give patients the benefit of a doubt by exploring not only the scalp, but the skull.

Dr. George A. Baxter, of Chattanooga.—In regard to trephining in cases of fractures without symptoms of compression, I desire to place myself on record as opposing operative procedures unless there are some indications for operation. I make the statement upon my own personal experience as opposed to what I know is the general tenor of thought in the profession. These operations under the aseptic method, and the little danger attached to them, carry us sometimes a little too far, and the pendulum is obliged to swing back in any surgical procedure, however small the risk may be. Even though the mortality may only be one per cent., as is claimed by some, we are not justified in trephining the skull without there is some indication for it, and these indications are obliged to be seen and known. We cannot have compression of the brain to any great extent without some symptom or symptoms immediately following it. If epilepsy exists; if there has been a fracture, and there are indications in the skull that will indicate in all probability where the injury is, then is

the time for operation. As a conservative man, I place myself on record as against an operation for which there are no immediate indications for remote effects twenty years off.

In regard to Dr. Nicolson's case, if he failed at the first time to have union of the ulna, I do not see any reason why he should not have cut down, reached the ends of the bones, and united them by ivory pegs, and thus get more or less bony union.

Dr. Roberts, of Louisville.—I regret that I was called away immediately after reading my paper, and did not have the pleasure of hearing Dr. Dugan's paper and very little of the discussion that has taken place. I take it, from what I have heard, that Dr. Dugan's paper dealt with depressed fractures of the skull, and the question was whether an immediate operation should be undertaken or not. I think in all cases where there is any evidence whatever of depressed fracture of the skull an immediate operation is absolutely demanded. If we wait until epilepsy sets in, so far as my experience goes, and I have operated on quite a number of these cases, very few of them are cured of their epilepsy.

I heard but little of Dr. Nicolson's remarks in connection with my paper, and just what the Doctor's line of argument was I do not know.

In the paper I read there were a number of cases in which several operations had been performed for ununited fractures, all of which had proved failures. There is a certain condition of the system which seems under certain circumstances to interfere with union. I think where we get a useful limb, that is all we can expect.

In connection with the suggestion that Dr. Nicolson made about the plaster-of-Paris, in my case, I will state that the man after the removal of the plaster used a Smith's splint for a month or so, and was able to walk with perfect ease, so much so that many surgeons advised against operative interference. Dr. Vance and I insisted upon resection, which resulted in a perfect cure.

Dr. Dugan, of Louisville.—In regard to the remarks of Dr. Baxter, I will say that if he waits for symptoms of compression coming on in ten days before operating, he will wait

until his patient is out of the surgical domain. Vascular changes will have taken place, and an operation will not relieve the condition. I am surprised to hear such conservatism. It explains why Dr. Briggs has so many cases to trephine for epilepsy ; as long as Drs. Baxter and Letcher continue to practice the kind of conservatism they have told us about, just so long will there be a necessity for Dr. Briggs to keep his instruments in readiness, because he is going to increase his list of cases right along.

I. SOME COMPLICATIONS OF OVARIOTOMY:

WITH CASES.

II. ABDOMINAL HYSTERECTOMY:

WITH SPECIMENS.

By L. S. McMurtry, M.D.,
Louisville, Ky.

I. Some Complications of Ovariotomy.

More than two years ago Mr. Lawson Tait said that the last words had been uttered on the subject of ovariotomy, the mortality having been reduced to the least of any major operation known in surgery. But ovarian tumors are so common and present such diversity of structure—dermoid, fibroid, parovarian, monocystic, polycystic, etc., with so many variations and complications—that they prove of continuous interest and importance.

There are very few major operations that can be confidently undertaken in very old subjects, operations where there will be a very heavy strain on the system, great danger of shock, and where there will be a great deal of traumatism to vital organs in connection with the operation; all dangers are very much exaggerated in the aged. The following cases I deem worthy of consideration:

Case I. *Ovariotomy in a Patient of Seventy Years.*—Last October I operated for a very large double ovarian cyst in the case of a lady from Kansas, seventy years old. The tumors were so large (I only suspected one tumor when beginning the operation) that they extended to the diaphragm; abdomen distended so that the skin was shining, the umbilicus pouting, and the general condition extreme and distressing. She said that

her journey from Kansas to Louisville was the most trying ordeal through which she had ever passed. The operation disclosed very extensive intestinal and bladder adhesions, which are always difficult to deal with, especially in cases where they are old and firm. The pedicle of the large tumor on the left side was one of the largest that I have ever dealt with; it had to be quilted with a double ligature and another ligature around. The patient has since made a good recovery.

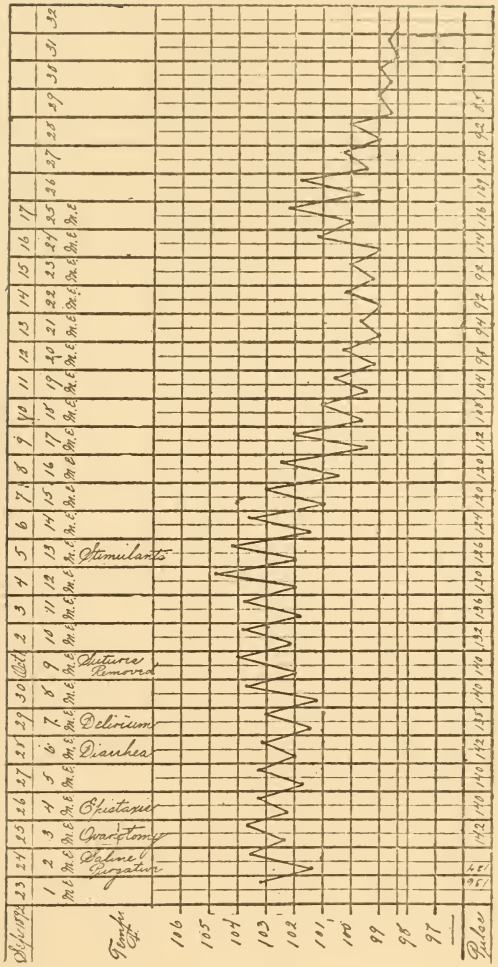
CASE II. *Ovariotomy in a Patient of Seventy-two Years.*—Another case, referred to me by Dr. J. M. Krim, of Louisville—a woman who will be seventy-two years old in March. When I first saw her with Dr. Krim we made a diagnosis of ovarian cyst as most probable, although neither he nor I could say positively that it was ovarian, because we could not get any fluctuation in the tumor. We had the patient moved to the infirmary; she was vomiting, with rapid pulse and high temperature; her abdomen was opened, and we found an immense cyst on one side, and, like the other case reported, a smaller cyst on the opposite side. The larger cyst was of a dark-bluish color; there were a great many adhesions, and new adhesions were forming. Peritonitis was active around it. After enucleating and tapping the larger cyst I found that the pedicle was twice twisted, the circulation being shut off from the sac; it was sloughing, and peritonitis was established. The interior of the tumor was filled with clots and dark colored fluid, the result of hemorrhage in the interior from obstruction of the circulation by this twisted pedicle. I removed both tumors; the peritoneum was thoroughly irrigated and drained, and she made one of the most prompt recoveries that I have ever known. There was not an untoward symptom of any kind.

These two cases are very interesting as confirmatory evidence of the fact that ovariotomy is the most successful major operation known to surgery. I believe that much of this is due to the fact that ovariotomy is largely in the hands of a few surgeons, and that the statistics of ovariotomy are furnished mostly by surgeons who have had large experience and have acquired results which can only be attained by skilful attention to all details of the operation.

CASE III. *Ovarian Cystoma with Twisted Pedicle and Peritonitis; Ovariotomy in Second Week of Typhoid Fever; Recovery.*—A young lady, twenty-one years of age, unmarried, was brought to me from Daviess County, Ky., the latter part of September, with ovarian tumor. Diagnosis was clear. She was very ill when she arrived, and when I took her temperature it was 103.5° F., with a rapid pulse. That night she had persistent vomiting, which on the following day became of that character which belongs to peritonitis, green, serous vomiting. On the second day I found her in such a serious condition, evidently from peritonitis, high fever with rapid pulse and vomiting, that I decided to operate at once. I found a very large ovarian tumor, extending up to the diaphragm, a cyst with double-twisted pedicle and sloughing sac. In separating the adhesions with my finger the sac ruptured, and the pus contained in the tumor ran out over the table. I should judge that at least two gallons of pus and serum ran out of the tumor. As is usual in these cases, there was a smaller cyst on the other side. The peritoneum was thoroughly irrigated with about three gallons of water, and the patient put to bed with very considerable shock. She reacted very well.

The next morning the nurse telephoned me that the patient was bleeding at the nose. I thought this was merely an accidental circumstance. When I took the temperature the morning following the operation it was 101.5° F., pulse still rapid. She had a red spot on each cheek, and presented a very unfavorable appearance. During the day delirium supervened, and that evening her nose bled again. The next morning examination of the body disclosed rose-colored spots; diarrhœa had come on, and I found that I had done an ovariotomy in the midst of an attack of typhoid fever. My friend, Dr. Thomas Hunt Stucky, confirmed the diagnosis of typhoid fever, and regularly visited the patient from this time on to recovery. She went through the course of this disease, but so far as the ovariotomy was concerned the result could not have been better had she been in a perfectly afebrile condition. She was five weeks in the infirmary. It was a very severe attack, the temperature reaching as high as 104.6° F.

She has now fully regained her health. I append the chart showing the pulse record and temperature observations.



II. Abdominal Hysterectomy.

There is no more interesting chapter in pelvic surgery than the development of supra-pubic hysterectomy for fibroid tumors of the uterus. The classical contribution of the elder Keith, in 1885, laid the foundation and stimulated work in this direction. During very recent times we have seen an old and a new fallacy dissipated. For many years it was taught

that fibroid tumors of the uterus were quite harmless; that they always disappear at the menopause, and that they are of no surgical importance. No one can now dispute the facts so commonly observed, that these tumors grow to enormous proportions after the menopause, and that they destroy life by hemorrhage, pressure, and exhaustion. The treatment by hypodermatic administration of ergotine, as well as by electricity and other tentative measures, has been demonstrated to be wholly inefficient, and more than this, to be dangerous by inducing peritonitis and adhesions.

The pathology of these tumors is now better understood, and improved knowledge has demonstrated the necessity of radical treatment. Fibroid tumors of the uterus are usually associated with disease of the uterine appendages—often suppurative—and this increases the danger many fold. The tumors themselves are subject to cystic and other forms of degeneration, and are especially prone to malignant changes.

With increased knowledge have come improved operative methods, and abdominal hysterectomy has within a few years advanced to a position which bids fair to approach the admirable status of ovariotomy. Indeed, there is every reason to expect such a consummation when operations are done earlier than heretofore, before complications and degenerations have rendered the results uncertain.

I here present two fibroid uterine tumors from two cases. The operations were both done during the past month. One is a large, single, soft œdematous myoma, resembling in size and form very closely the gravid uterus. The other tumor is a hard, nodular myoma, which has undergone degeneration and suppuration. The first tumor drove the patient to seek relief on account of the severity of pressure symptoms, the latter was removed in the midst of an attack of peritonitis. Both patients are now in the infirmary in advanced convalescence. Both pedicles were treated by the extra-peritoneal method, with the *nœud*, and both pursued an ideal post-operative course. In neither case did pulse or temperature pass beyond the normal.

I shall make these cases the text for discussing the mooted question of the hour in abdominal hysterectomy, viz., the best method of dealing with the pedicle. There are practically three methods of treating the pedicle: (1) to fix it in the lower angle of the parietal incision; (2) to provide against hemorrhage by ligature and suturing, and drop it as in ovariotomy; (3) to completely excise the cervix. It is sufficient for this discussion that vaginal fixation of the stump be considered in connection with the second method.

Total extirpation of the cervix has received a positive impetus recently by the successful results of Ross, Reed, Polk, Eastman, and Hall. The intra-peritoneal pedicle is the operation more especially of the German school, while Bantock, Tait, and Price are the leading exemplars of the extra-peritoneal method with Kœberle's *nœud.* This was Thomas Keith's method. That good results are attained by skilled operators with each of these methods is presumptive evidence that all three are practicable. In my own cases (seven in number) I have used the *nœud* exclusively, and up to the present time all my cases have recovered and all the patients are in the enjoyment of health and activity.

The great dangers to keep in mind in abdominal hysterectomy, besides sepsis, are shock and hemorrhage. Injury to bladder and ureters are ever-present dangers during operation. Everything which prolongs anæsthesia and protracts the operation tends to beget shock. The operation with the *nœud* is the easiest to execute and requires less time; hence shock is reduced to a minimum. I pause here a moment to say that I know nothing more remote from the truth and facts than the common talk that this method is "unsurgical," "crude," "barbarous," etc. When the *nœud* is properly applied to a reduced pedicle it is as surgical and elegant as any operative method whatever. Scientifically considered, the *nœud* is nothing less than a wire ligature adjusted under absolute control and guaranteeing safety from hemorrhage.

The reduced pedicle, with the delta *nœud* in accurate adjustment, and collared securely with the peritoneum beneath

the wire, is altogether different from the huge mass as large as the leg encircled with wire, and left to slough, which seems to be the conception of the extra-peritoneal pedicle in the minds of many. No one who has not mastered the making of a pedicle by deperitonization and reduction as practised by Price, can do justice to the practical application of the *nœud* in hysterectomy. When so treated and the pedicle is brought well down, it is free from traction and control, and I can demonstrate to-day that it remains dry and without suppuration throughout.

This method is applicable to all sizes of tumors, and is readily applied in those cases where the tumor rises out of the pelvis and lifts with it the pelvic peritoneum and broad ligaments. This method commends itself to confidence by its results.

The objections to all methods which necessitate ligature of ovarian and uterine arteries separately, with extensive suturing at the floor of the pelvis, are that the dangers of operation are augmented and the difficulties unnecessarily increased, while dangerous sequelæ and complications are invited in this, as in all operations, by complicated methods.

In conclusion, I would protest against the common practice of alluding to the extra-peritoneal method of treating the stump as "crude." Surgical operations are not to be regarded as mathematical exercises, but should elicit the application of the most exact knowledge to the great purpose of saving human life.

DISCUSSION.

Dr. Henry O. Marcy, of Boston.—I am deeply interested in the subject so well presented by Dr. McMurtry. I can only briefly refer to the subdivision of it—how best to treat the large tumors which are practically without a pedicle. These too often completely fill the pelvic cavity, and by no method yet practised can be treated by bringing the stump or base of the tumor externally. They are without a pedicle. Dr. Eastman, whom I am glad to see present, and to whom you will be delighted to listen, has devised methods for the complete removal of the uterus with

the tumor which merit very favorable consideration. By the use of the reverse, or Trendelenburg position of the body, this may often be effected with a certitude and safety which are surprising. The cervix uteri is of value as a point of fixation and support to the bladder and rectum which often necessitates the preservation. In order to do this and leave a safe intra-peritoneal pedicle I have for some years been in the way of utilizing a portion of the peritoneal covering of the base of the tumor. The method is briefly as follows, and was suggested to me, some years ago, by the device first taught by Dr. Miner, of Buffalo, in the treatment of ovarian cystoma, when the general practice was by clamp or ligature, the pedicle being retained externally. He called it "stripping the pedicle," *i. e.*, removing the cyst from its peritoneal envelope, which was retained in part or entire.[1]

Upon the sides of the tumor, over its lower border, divide the thinned-out envelope and enucleate, after having firmly sutured the vascular supply on both sides of the uterus, well down into the broad ligaments. Reform the parts to be coaptated, as deemed fitting, and within the peritoneal borders close the entire base with a line of continuous double sutures. Cover these by intra-folding the peritoneal envelope by means of a continuous parallel suture, taken three or four lines external to the cut edges. The suture commences a little below the limit of the wound, and the stitches should not exceed one-fourth of an inch in length. It is introduced upon the distal side, exactly opposite to the emergence of the preceding stitch, and is so continued until the wound is completely closed. It will be observed that the suture crosses the wound at each stitch, exactly at right angles, and when drawn upon, completely and accurately, closes the approximated edges, while it is itself entirely buried. When thus closed there is no open wound left within the abdomen, no possibility of hemorrhage—a pelvic cavity, clean and aseptic, permitting a restoration of the structures to their normal positions, and a safe closure of the abdominal wound. These are manifold advantages, which often, even in the most difficult cases of large myoma, are productive of conditions nearly ideal, leading to safe and easy cures.

[1] The Surgical Treatment of Non-Pedunculated Abdominal Tumors. Journal American Med. Association, Nov. 8, 1890. Reprint.

Dr. Howard A. Kelly, of Baltimore.—Dr. McMurtry has brought before us in his paper three distinct topics; I will briefly discuss two of them.

In the first place the Doctor has the good fortune to place on record an indubitable case of typhoid fever complicating ovariotomy. His patient had *fever*, and a *quickened pulse* before operation, she had *epistaxis* and *rose-colored spots*, and a typical *temperature curve* defervescing to recovery, associated with the characteristic *diarrhœa*. No one can question the diagnosis. Most cases of "typhoid fever" reported as following abdominal operations are simply cases of sepsis, and the patients die of sepsis in a *typhoid condition*, and not of typhoid fever.

Analogous to this error are cases reported as dying from heart disease after an abdominal operation. Fenwick, of London, has reported a series of cases in *The Lancet*, where it is surprising to note the number of patients dying in consequence of fatty degeneration of the heart. Septic cases almost always exhibit fatty degeneration of the heart, due to the sepsis following the operation, and not due to the large abdominal tumor, and I think some of these are explained in this way.

Changes due to large tumors may occasionally be of another character—hyaline degeneration. For a case of this kind see my fasciculus in the *Johns Hopkins Hospital Reports*, vol. ii, Nos. 3 and 4, "Gynecology," Article III., p. 168.

The second group of cases Dr. McMurtry discusses are those small pelvic masses which occasion such serious distress to so many patients, diseases not often recognized by the general practitioner and yet of extreme importance. I do not know how we can better educate the profession in this matter than by dwelling here upon a few of the prominent symptoms, and by demonstrating the signs in our clinics, showing the fixation of the uterus, with the hard masses laterally, which to a trained touch are so distinct that a diagnosis can often be made in less than a second by a single sweep of the finger.

There is an impression on part of the laity, and even many doctors, that the removal of these small pelvic tumors means the removal of ovaries and tubes capable of a normal exercise of their functions. This is a gross error, although such a criticism might have been pertinent eight or ten years ago. Every right

minded man will earnestly condemn the removal of healthy appendages except under extreme and rare circumstances. The following letter I have recently received, although not logical, is the best ethical statement of the case I have seen, and it will be better appreciated as coming from a layman :

"MY DEAR DR. KELLY: Mrs. A. tells me that in your specialty of surgery you are criticised for 'unsexing women.' Will you permit me as a husband and as a priest to give you my opinion of that for which you are criticised.

"While ovariotomy does not destroy sexual desire nor the pleasure of cohabitation, yet the removal of the organs of motherhood causes a serious obstacle to the affections due a wife. For, in depriving a woman of the possibility of children, there is taken from the home the unifying power of parental love. And no high-souled affection can be sustained by mere sexual pleasure where the hope of children is taken away. And every Christian husband who understands God's chief purpose in marriage—namely, reproduction of species—cannot justify marriage as merely the means of sexual gratification. As a husband I believe that neither life-long helplessness nor anything short of impending death justifies ovariotomy, if with the diseased organ or organs remaining there could be the remotest reasonable hope of children. For the woman pain of body is preferable to the anguish of soul attendant upon the destruction of the hope of becoming a mother. And, as a man, I should, in my present light, conscientiously decline to marry the best of women from whom had been taken the sacred function of motherhood.

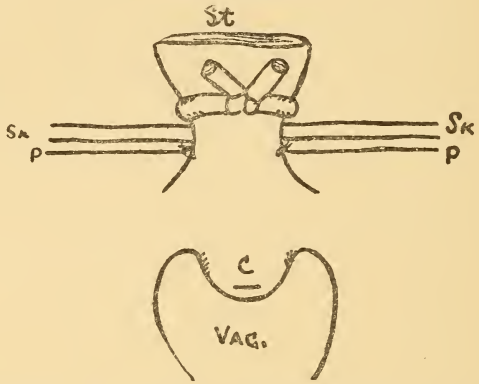
"As a priest I believe that the absence of that function excludes the right of marriage and the right of sexual cohabitation, except where that act is needful to prevent mental impurity or the sins of adultery or fornication. Since the diseased state of the ovaries taken from Mrs. A. before marriage ratified your prognosis, and you were wise in having my consent to the operation and the assurance of mutual agreement between the betrothed, that the second operation in which the remaining ovary was removed was based upon the fact that Mrs. A. would be better fitted for a life of usefulness as a clergyman's wife—your operating in this instance was absolutely above criticism. At the close of about ten years you continue to stand at the bar of my

judgment, both as a husband and as a priest, absolutely without reproach in the operation of ovariotomy upon my wife. And you are at liberty to use this letter wherever it may tend to justify yourself and the special surgical skill with which God has endowed you, with but such limitation as your honor as a Christian gentleman and physician places you in protection of Mrs. A. from unpleasant publicity as to the fact of ovariotomy having been performed upon her.

"Believe me, with profound respect
"Yours very faithfully."

As to hysterectomy, I believe that the best results in abdominal surgery in the world to-day are those of American operators, and

Fig. 1.



St. The pedicle. Sk. The skin. P. The peritoneum. C. The cervix. Vag. The vagina.

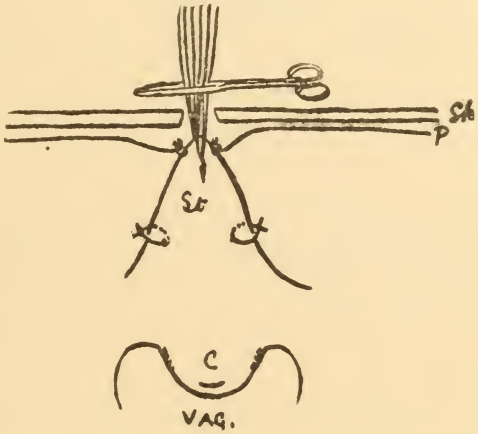
this is well demonstrated in the field of abdominal hysterectomy by the low mortality.

We should bear in mind in discussing this question that we are not striving for the same end we were five or six years ago. The question now is no longer how to diminish the death-rate, but "what form of operation occasions the least immediate discomforts and the best remote results?" There are four general types of these operations, four methods of treating the stump in large myomatous tumors. In two of these the pedicle is suspended; in one it is dropped, in the last there is no pedicle at all. I will

illustrate these methods on the blackboard by the accompanying Figs. 1, 2, 3, and 4.

If you will look at Fig. 1 you will see the old method of treating the stump by ligating it *en masse*, and bringiug it up out of the abdominal cavity, where it is treated aseptically, and in from ten days to two weeks sloughs off, leaving a pit in the abdominal wall to fill up by granulation. In view of the much better methods which we now have at our command, this procedure has become unscientific and unsurgical, as unsurgical as it would be to cut off a finger by holding it in a clamp and screwing up the clamp every day until the amputation was completed.

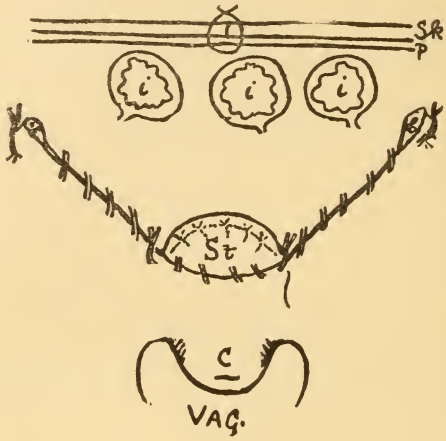
Fig. 2.



The second method devised by myself has proven invaluable as a safe step or transitonal stage toward the third method. In this method I used to close up the stump entirely as if it were to be dropped, and thus did away with the constricting ligature or the serre-nœud. However, to be sure that the dangers of hemorrhage and sepsis were obviated, I suspended the stump deep down in the lower angle of the wound, where it was simply attached to the peritoneum on all sides. This method dispensed, then, with any sloughing, did away with the tension often necessary in dragging a short stump outside of the abdomen in the first method, and allowed me to keep the whole wound area constantly under control, preventing any septic trouble or hemorrhage getting into the

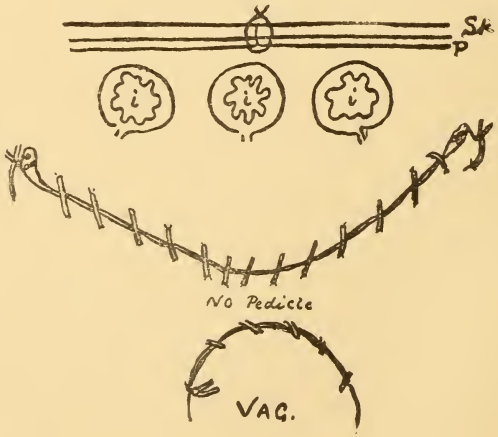
abdominal cavity. I soon found, however, that neither sepsis nor hemorrhage was liable to occur, and I was emboldened to drop

Fig. 3.



my stumps as after ordinary ovariotomy. This is the method figured in the third diagram, which I have now been following for some time with great success.

Fig. 4.



I have further illustrated in the fourth figure the complete removal of the uterus and cervix, leaving no pedicle at all. I prefer myself the method figured in the third diagram.

We should remember, in performing these operations, that the tumor in common with the uterus is fed by four main trunks of bloodvessels, the two uterine and two ovarian arteries. If we succeed in ligating these trunks well away from the tumor there can be no serious hemorrhage. I would refer here to an article on this subject published in the *Gynecological Fasciculus of the Johns Hopkins Hospital*, 1890, vol. ii., Nos. 3 and 4, entitled "Ligature of the Trunks of the Uterine and Ovarian Arteries as a Means of Checking Hemorrhage from the Uterus and Broad Ligaments in Abdominal Operations." The steps of an abdominal hystero-myomectomy as I now perform it, are briefly these:

First. A long abdominal incision, exposing the tumor.

Second. Ligation of the ovarian vessels above the brim of the pelvis, controlling at the same time the collateral circulation from the uterus by clamps and ligatures.

Third. Cutting boldly down through the broad ligaments beneath the tumor, doubly ligating the round ligaments.

Fourth. Pulling apart the anterior and posterior layers of the broad ligament, and pushing down into the cellular tissue at the floor of the pelvis, where the uterine arteries are discovered and ligated.

Fifth. Cutting away the tumor and uterus above this point, severing the peritoneum in front, just above the vesical plica.

Sixth. Burning out the canal and closing up the cervix, reduced to small dimensions, by buried catgut sutures.

Seventh. Whipping together the layers of the broad ligaments on both sides, covering in the top of the united cervix with the vesical peritoneal plica, left long in front for this purpose.

Eighth. The concluding steps of the operation—careful inspection of the field—observing any hemorrhage, cleansing the peritoneum, and complete closure of the abdominal incision.

Dr. Joseph Eastman, of Indianapolis, Ind.—I have only a few words to offer in explanation, since my name has been mentioned here. In some fifty-nine supra-vaginal hysterectomies I have gone over the ground thoroughly, treating the pedicle intra-peritoneally according to Schroeder's method and extra-peritoneally according to Bantock's method. I have made a great

many tests, and some time ago I published a method of my own which I had been using with excellent results. At that time Dr. Ross, of Toronto, had some correspondence with me, and secured the instruments I had invented and had been using. He said he had been much pleased with them, and since then other gentlemen have been using them. But for several reasons I have been encouraged to change that method a little.

I find that Drs. Kelly, Baer, and myself are not so far apart in our methods. We are all trying to reach the same result. In one case, as I supposed, I removed the entire cervix. I found, on examination later, half of the vaginal cervix remaining. The case recovered. At another time I left quite a portion of the cervix, cutting into the posterior cul-de-sac, dragging the ligatures out behind the cervix, and in this case the patient also made good recovery.

After studying carefully the matter of vaginal irrigation, scrubbing, etc., and finding that the vagina did not remain as thoroughly aseptic as I had expected (poisonous substance being forced into the vagina by manipulation of the tumor), I have come to the conclusion that if we can, in any way, avoid opening the vagina we have gained a great point. I would no longer attempt to drain the peritoneal cavity by the vagina. It is not so rational so far as I understand the subject. A crooked glass-tube passed up through the vagina, turned down to the bottom of the cul-de-sac, was observed about three hours after the operation. I found that by the act of vomiting and retching the tube had been turned upside down, and did not drain. Vaginal drainage is a source of vaginal infection, and instead of "a torch over the powder magazine," I think vaginal drainage of the peritoneal cavity is a torch under the powder magazine.

Drainage of the peritoneal cavity out of the abdominal wound is much better than vaginal drainage. Retching and vomiting will force the fluid out the glass-tube placed in the abdominal wound. I have come to the conclusion, first, that it is no longer necessary to take out the entire cervix; second, that we increase the danger of vaginal infection and do that which is entirely unnecessary. We can leave the vagina shut off from the peritoneal cavity; third, we should leave only the vaginal portion of the cervix, and that but a shell. The necessity of taking it out does not equal the

danger of vaginal infection. If the broad ligaments are brought well down into the wound and covered with peritoneum, there will be no prolapse of vagina following.

Dr. C. A. L. Reed, of Cincinnati.—I take great interest in this discussion, particularly the part pertaining to hysterectomy, because I believe we are now working on convergent lines, and that the ultimate result will be a perfected operation which will reduce the mortality from this procedure to a minimum.

With regard to the various procedures, I may say that I have operated by the *nœud*, I have seen it used by Bantock, Tait, and other European operators; I have seen it used in New York, Philadelphia, and in my own city. I have watched with extreme care the process of so-called mummification; we are told that it is a process of shrinkage, of non-septic necrosis; that it is essentially a dry process, for the reason that there is constriction at a particular point, and that the distal extremity of the pedicle perishes by virtue of the process of mummification. Attention is not called to the fact that there is beneath the point of constriction a circulatory cutting process upon one side of the wire more than upon the other. I have seen the fluid that comes from around the pedicle in the practice of myself and others; I have seen fluid which is non-septic in character. I have also seen pus coming from around the stump, having the creamy consistency characteristic of it. While that is going on we are practising a procedure which is not in accordance with the canons of modern surgery, consequently this operation must go. It is destined to go by the law of progress.

Dr. Eastman has been instrumental in developing a technique which I believe goes a long way toward specially solving the problem, and I beg leave to allude to the technique in certain of these troubles. I have had no difficulty in dissecting out the entire cervix; that is a small portion of the operation. The difficult part is that to which Dr. McMurtry has alluded with such force and accuracy, that is, the adjustment of the ligatures to the sides of the uterus. The dissection is one that I have been able to carry out with perfect ease and facility without the assistance of the uterine elevator, an ingenious device of Dr. Eastman.

We come to the question of closure of the vagina. Dr. East-

man has taken a long step forward when he advised against drainage in the vagina. I am surprised that he yet clings to what is to my mind an inconsistency, taking his long ligatures out through the vagina, because he has then performed a sort of capillary drainage, the current which he cannot control, which is liable to cause infection. Cutting the ligatures short, reaching down and catching the mucous margins of the upper extremity of the vagina, approximating them, two or three deep sutures can be passed through peritoneum, through cellular tissue, and the whole vagina cut off, and the procedure of cleansing need not take ten minutes when the Trendelenburg posture is employed.

Dr. Rufus B. Hall, of Cincinnati.—I am of about the same opinion as the other gentlemen who have thus far participated in the discussion. There are many objections to the extra-peritoneal method of treating the stump, one of which I desire to emphasize. If a wire is used, all must confess that we have necrotic tissue above it as the process of separation goes on. We must have that; we must learn our pathology anew if we entertain for a moment the idea advanced by our opponents, that we can have necrotic tissue above the wire and separation of the dead tissue from the stump and have no danger of sepsis. Every operator who uses the extra-peritoneal method with any of its modifications has been exceedingly fortunate if he has not had sepsis from this cause. We must recognize it as a source of danger of no small magnitude. There are other reasons why the extra-peritoneal method is not an ideal one. I have resorted to this method in a dozen or more cases. My statistics are not satisfactory to myself, although I am not ashamed of the results. Any method which is not an ideal one will stand improvement, and ought to be investigated to see if there is anything better. When the total extirpation method was brought before the profession by Dr. Eastman I resorted to it, and I desire to give Dr. Eastman credit, and to say that it was a step in the right direction. It brings us back to an ideal method. Whether or not this method will give us the results the extra-peritoneal method now gives I am unable to say, yet it promises even better. I am here to advocate total extirpation, and I am going to do this operation until I have a greater mor-

tality than from the extra-peritoneal method. I have seen Tait, Bantock, Schroeder, Billroth, Brown, and a great many others make the extra-peritoneal operation, and they have all had sepsis and many other accidents following it. When we have before us the total extirpation method of Dr. Eastman, in my opinion we have an ideal one.

We know very well in the operation for ovarian tumor, dropping of the pedicle had its opponents, and bitter ones they were too. We hear no more about it. We do not have hemorrhage unless it is accidental, which is avoidable as a rule.

In the intra peritoneal method of treating the stump, cutting away the broad ligament, so beautifully illustrated by Dr. Kelly, we place our ligatures on exactly the same structures which we place them in making ovariotomy. When Dr. Kelly had everything done which he proposes, why did he not go one step further and get rid of the cervix? What good could it do? He could have removed it in less time than it takes to do what he proposes. They say that this acts as a key to the arch; it prevents prolapse of the vagina. Have you ever recalled the fact that in total extirpation for cancer, how few patients have herniæ? I have never heard of a case yet. We have a better condition of the parts in the Eastman method, a more thorough operation and a better condition for repair than we have in total extirpation of the uterus for cancer. Then why should we have hernia in the Eastman operation?

Dr. Reamy.—I think Dr. Eastman leaves the cervix.

Dr. Hall.—I would like to ask Dr. Eastman whether he leaves the cervix or not?

Dr. Eastman.—Just a little bit of it.

Dr. Hall.—Will you tell us for what purpose you leave this little bit of cervix? Do you leave it to shorten the time of operation, or as a benefit to the pelvis, to prevent hernia?

Dr. Eastman.—I leave it to *save time.* I do not want to make a hole and then have it to sew up again. I do not leave any more of the cervix than I can represent with the tip of my finger.

Dr. Hall.—Then he practically takes the cervix out. Regarding the manner in which the ligatures are placed on the broad ligament, they should be left long and brought out

through the vagina. I have made the operation but a few times, yet I am convinced that the method of leaving short ligatures is an error, one which any man who follows up will regret, for the reason that there is some danger of infection from ligatures. If you have infection you will have pus, and if you have pus you will sepsis have and probably death. In my second case I cut the ligatures short, but on the twenty-first day the patient commenced to get feverish, followed by a discharge of pus. [Here Dr. Hall illustrated on the board his method of operating.]

Dr. Thad. A. Reamy, of Cincinnati.—This question, as it now stands before the society, ought to be a discussion upon the relative merits of total extirpation of the uterus, including, of course, the cervix, and the merits of partial extirpation, making a pedicle of the cervix and placing it extra-peritoneal or intra-peritoneal. I make this statement because the method advocated by Dr. Hall, who has just preceded me, is total extirpation of the uterus. For, notwithstanding the fact that he amputates the uterus from the cervix, he subsequently removes the cervix. If I have not misunderstood what I have read and heard as to the method of Dr. Eastman, of Indianapolis, who is now present, and I hope will speak for himself, he sometimes removes the entire cervix and sometimes leaves a portion of it. In this particular, if I remember correctly, he agrees with that distinguished operator and teacher Dr. Polk, of New York. Baer, of Philadelphia, in all cases, leaves the intra-vaginal portion of the cervix. I am not prepared to say that the time will not come when total extirpation, or amputation at the upper portion of the cervix; will not be the established operations. Most assuredly, if time and experience shall prove that by these methods the largest number of lives can be saved, then they will be adopted. At the present time, however, I do not think my friend, Dr. Hall, is justified in the statement that "extra-peritoneal treatment of the pedicle is a thing of the past," denouncing it as he does as unsurgical. By this method the last reported 38 operations by Mr. Keith resulted in 2 deaths and 36 recoveries. Kaltenbach, 22 operations and 1 death. Bantock 22 operations and 2 deaths. Thus we have mortalities of 9 per cent., $4\frac{5}{10}$ per cent., 5 per cent., $11\frac{7}{10}$ per cent., etc. In his more recent operations McMurtry has obtained equally good results. Price, by the

same method, seems to have secured a mortality of only 3 per cent. in a large number of operations. In my own earlier work I had 3 deaths in 13 operations for removal of large fibroids, extra-peritoneal treatment of the pedicle. So that, if others had not secured better results, I should not be so ardent an advocate of the method. Without selected cases I have had a run of four successful operations.

It seems to me that these gentlemen beg the question when they allege that the success attending extra-peritoneal treatment of the pedicle has been due to the extraordinary skill of the operators, despite their methods. No degree of skill can wholly overcome radically faulty methods. It must, of course, be acknowledged that Keith, Bantock, Kaltenbach, Tauffer, Price, and McMurtry are skilful operators. But the same must be said of Martin, Olshausen, Dohrn, Gusserow, Leopold, Eastman, and others who employ the intra-peritoneal method. And yet by the latest statistics in a large number of operations by the internal treatment of the pedicle, the mortality is 25.8 per cent., whereas by the external method it is 21.6 per cent. So far as statistics testify, therefore, showing the work of equally skilful operators by the two methods, results are favorable to the extra-peritoneal. Again, I repeat, however, that further experience may entirely change the situation. No one shall be more ready than myself to accept it. Inadvertently I omitted to quote, in its proper order, the ingenious methods and high results which have been obtained by our distinguished member, Dr. Kelly, of Baltimore. Finally, it is not true, as alleged by a previous speaker, that necessarily, after external treatment of the pedicle there is prolonged and dangerous suppuration. If it is properly mummified outside of the wire at the time of operation, and proper attention given to tightening the wire, frequently there is almost no suppuration. I have recently secured such results and seen it in the practice of others. If the peritoneum is properly closed and stitched to the pedicle below the point of constriction, and no drainage-tube is inserted, which is the proper practice, even if there should be slight suppuration at the point of constriction, it will not infect the peritoneal cavity, and will not prove a source of danger.

Dr. G. Frank Lydston, of Chicago.—Dr. McMurtry's case

of ovariotomy in the second week of typhoid fever brought to my mind a very interesting case I saw with my friend, Dr. Senn, about three or four weeks ago, in which he had performed an ovariotomy upon a woman at about the same period of typhoid fever, and did the operation deliberately and without any idea of having any particular complications incidental to the typhoid; in fact, he said to me that he would about as soon operate upon a woman in that condition as under ordinary circumstances.

There is no question but what my friend, Dr. Kelly, is *au fait* in the matter of abdominal surgery; but as the Irishman said, he is a little *au fat* on the subject of fatty degeneration. He made the broad assertion, which I think ought not to go without contradiction, although it has no particular bearing on the question of abdominal surgery, that fatty degeneration of the heart, incidental to large abdominal tumors, is not due to the tumor, *per se*, notwithstanding the fact that everybody recognizes the circumstance that malnutrition almost necessarily results from these large abdominal tumors of long standing. Malnutrition is one of the primary causes of fatty degeneration. I infer from what has been said that my abdominal friends lose very few cases from sepsis in abdominal surgery; so I infer that Dr. Kelly's assertion of sepsis as opposed to the idea of fatty degeneration from malnutrition, is based on comparatively few cases. It is well known that fever will produce fatty degeneration of the heart, and one of the disastrous results of prolonged fevers is malnutrition of the heart with resulting fatty degeneration. The experiments of Bernard and Senator upon dogs demonstrated this fact conclusively. It has been shown, also, that general nutrition is considerably impaired by extreme heat. It is illogical in my estimation for any man to formulate the broad proposition that septic infection of short duration is always sufficient to account for fatty degeneration of the heart muscle. It requires prolonged disturbance of tissue nutrition and a marked disturbance of tissue metamorphosis to bring this about.

Dr. Reamy perpetrated a heresy, I think, when he said that a granulating surface would not absorb. I wish that he could prove his assertion to the satisfaction of everybody.

Dr. Reamy.—I do not think an ordinary granulating surface does absorb.

Dr. Lydston.—Granulation is always associated with more or less absorption of pus products. To show the readiness with which a granulating surface will absorb it is only necessary to inject a solution of methyl-blue into an abscess cavity or apply it to a granulating wound. In a short time the granulations are extensively stained. Indeed, the surrounding tissues are soon stained for a considerable area. The sudden occurrence of septicæmia as a result of infectlon of granulating wounds must be remembered in this connection.

Dr. W. C. Dugan, of Louisville.—In regard to suppuration of the pedicle, that point has not been made clear. There was a time, a few years ago, when pus was thought to be invariably the result of micro-organisms. I feel confident the investigations of Dr. Roswell Park, of Buffalo, in the Mutter lectures on "Selected Topics in Surgical Pathology," before the College of Physicians of Philadelphia, have pretty well disproved that theory. He claims, without pretense to priority, that we can have pus without the presence of bacteria. In those cases Dr. McMurtry has spoken of, the suppuration is the thrown-off dead phagocytes, consisting of dead connective-tissue corpuscles. If you were to make a microscopical examination of the same you would most likely not find the microbes of suppuration. If bacteria be found at all, they are the non-pathogenic variety, and if these organisms should pass into the abdomen septic peritonitis would not follow. In those cases where there is no evidence of bacteria you would not expect to have peritonitis. The creamy matter which we see is not necessarily pathological pus. It is puruloid material, and not real pus in a septic or pathological sense.

Dr. McMurtry.—One important indication for the extra-peritoneal pedicle is the security afforded as to hemorrhage. This is the great danger after hysterectomy, and by this method the pedicle is under constant observation and absolute control. Concerning Dr. Marcy's method of complicated suturing and Dr. Kelly's method of ligature of arteries and suspension of pedicle, I desire to say a word. Both these surgeons are graphic and fluent of speech and skilful of hand, and the methods they have described will not be found simple and easy when tested at the operating-table. Moreover, both methods protract the operation,

prolong anæsthesia, and thereby increase shock. The method of complete excision, as practised by Reed, Hall, Eastman, Polk, and Ross, is open to the same objection. Again, the danger is increased by opening the vagina, and by suturing across the pelvic floor, exposing to sepsis. The ligation of uterine and ovarian arteries is not so easy as one might suppose from the descriptions we have heard.

When one has learned to strip the peritoneum from the tumor, letting down the bladder in front and rectum behind, placing the *nœud* around the surgical neck of the uterus, and bringing the reduced pedicle well down in the lower angle of the wound, the peritoneum within the pelvis is intact and hemorrhage under perfect control. I have never seen a tumor in which this method could not be applied, and this is the testimony of Price and Bantock, whose experience is very large. This method, when once thoroughly understood, is the easiest of execution and yields the best results. I do not doubt that the other methods described are admirable in the hands of those who have practised and perfected them, but that method is best which cures the patient most surely with most safety, and which can be readily and quickly applied.

INTESTINAL ANASTOMOSIS BY A NEW DEVICE.

By H. Horace Grant,
Louisville.

A word only is needed in the presence of this Society to indicate the paramount importance of establishing some satisfactory technique of intestinal anastomosis. It has come to be recognized that the operation has no substitute, and also that even in situations specially suited to them, neither circular enterorrhaphy nor implantation are to contest for election. The older history is of little value, and the recent methods are familiar to you all.

After considerable experimental work on dogs, I found the anastomotic opening, an inch to an inch and a quarter, could be established in small animals in from fifteen to twenty-five minutes, with almost uniform success, by the use of raw-hide plates modified after Robinson's suggestion. In a recent paper I reported over fifty cases of such operations, and expressed a belief that after the technique described satisfactory results would be obtained.

I recognized two serious difficulties in the making of anastomotic openings by any form of plates, viz.: That of getting a sufficient opening without the plate becoming unwieldy, and in the second place, the danger of septic infection of the peritoneum by the threads of the plate. The first objection I thought the rawhide plates—easily obtained and promptly absorbed after use—would overcome; the second I look on as largely theoretical, and perhaps unavoidable. A few months after the publication of this paper, I read, with great interest

the report by Abbe[1] of three successful cases by direct suture. I note his criticisms of mechanical devices and his suggestions. By post-mortem examination of patients dead some months after successful anastomosis is disclosed the contraction of the opening to about half its original size. Abbe declares it is not practicable to employ bone plates to make the opening long enough, and that other devices are unreliable, that the danger of leakage by the side of the plates is of moment, and that the time gained is too inconsiderable to weigh; moreover that the appearance and security of an anastomosis by direct suture is out of all comparison.

This direct suture Abbe claims to have accomplished in twenty minutes. This method involves three rows of sutures around a four-inch incision; each row would be some ten inches in length, equalling thirty inches of Lembert sutures under very difficult surroundings—together with ligating, irrigating, and closing the divided gut ends—to say nothing of opening and closing the abdomen.

Very few surgeons would offer to accomplish it in less than an hour, and only very considerable practice, added to special skill, could hope to do so in half that time. I could not be got to underestimate Dr. Abbe's skill nor to undervalue the practical worth of his splendid paper, but rather to offer a comparison which indicates at what a disadvantage he has the great body of operators who may essay the plan. A very large proportion of such operations must be in a measure emergencies—time is not permitted always to get skilled experience, without which direct suture as herein suggested is hopeless.

The great bulk of the profession is in earnest search for aid and security in this particular emergency, and must look for it outside the lines of special equipment and skill.

It is a most dangerous thing to underestimate the importance of the many little essentials in any capital surgery—easily as they may be executed by the experienced and skilful.

[1] N. Y. Med. Record, April 2, 1892.

Only in the hands of genius are the first surgical operations without mistake and awkwardness, and such mistakes and awkwardness mean almost certain death in operations in the abdominal cavity. Successful intestinal surgery is difficult of accomplishment always—not alone that wise and educated judgment is needed, but dexterity in a particular line only can hope to promptly accomplish the end. Unquestionably it is easier to make intestinal anastomosis by the plates—other things being equal.

Whether these plates can be safely used to make the opening so large as Abbe declares necessary in the adult has not been tried. I think it would be very unsafe to attempt to approximate four inches of incision in this way; there would be great danger of leakage, and the bulk would be troublesome. It would be less dangerous to make the opening shorter and elliptical. I have never seen any sloughing in the gut walls after an elliptical opening one-quarter of an inch wide. Considerable portions are constantly removed after lacerated wounds of the intestines and stomach, and in operations for malignant growths of these viscera, without sloughing; still it is possible, and surely the limit of safety would be between a quarter to a half inch diameter of the ellipse. The length could be one and a half to two inches without increasing the risk; the cicatricial contraction would be proportionately much less. I am at present conducting some experiments to determine the amount of contraction in proportion to the diameter and length of the ellipse. For more than a year I have been endeavoring to perfect some instrument after a suggestion by Dr. J. T. Blackburn, to simplify direct suture. It has been so difficult to get just what I wanted that time has not been allowed since the completion of the instrument to test it fully. It is to be used only after resections. The two blades of the clamp are scissors, two and a half inches long. The arms of the clamp are made long enough to allow of introduction full five inches. After the gut is exposed a strand of iodoform gauze is passed through the mesentery and constricts

the intestine fully six inches from each point of intended resection.

The mesentery is tied off over the portion to be resected, with fine silk, in two inch loops, cut close and dropped in the usual way ; no V-shaped portion should be taken out, as such a step is a waste of time. When the resected portion is removed the gut ends may be washed out if desired. While the two ends of the divided intestine are held parallel, one blade is entered in each, allowing at least one and a half inches of gut beyond the proposed anastomotic opening to permit of invagination of the ends. The clamp is tightened, and the two surfaces thus firmly held are rapidly stitched together by a continuous overhand Lembert suture of fine silk. Two rows of parallel sutures, as suggested by Abbe, may be used if desired, though it has seemed that one is enough in my few experiments. This work can be done far more rapidly and accurately than without the clamp. When the suturing is finished the clamp is tightened, if need be. The scissors action of the blades, together with the ten or fifteen minutes pressure, prevents any hemorrhage. The clamp is now opened and withdrawn, the forefinger passed in the opening to assure its patency. The ends are now invaginated in the usual way. A severed omental graft may be employed if indicated. There can be no question but that a fair test of the usefulness and safety of any method of intestinal surgery cannot be got in experiments on animals, though much valuable information has been gained in this way. I hope in a few months to offer some more definite answer to the two points indicated in this paper.

The safety and utility of the elliptical opening.

The feasibility and value of the clanp method here suggested.

It has seemed from my experiments that the ellipse will make the plate method successful and easy in anastomosis without resection, and the clamp add greatly to aid direct suture after resection.

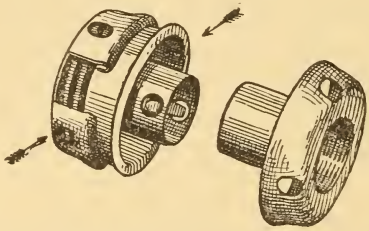
DISCUSSION.

Dr. A. V. L. Brokaw, of St. Louis.—I have had considerable experience in the matter of anastomosis, and have done a good deal of original work. I have tried all the methods of anastomosis with the rings, plates, etc. I wish to compliment Dr. Grant on his interesting paper. It is a step in the right direction. Any device that will materially assist us to do an intestinal anastomosis rapidly we should hail with delight, inasmuch as in these cases time plays an important part. I believe resections can be made more rapidly with Dr. Grant's device than without it. I shall take pleasure in conducting a series of experiments with this instrument, because I believe it is a good thing, and will be of material assistance in performing intestinal operations. The methods we have used in the past, making use of rings, plates, mats, or other foreign bodies in the intestinal tract, are bad. While I have devised a segmented rubber ring for use in intestinal surgery, I do not think it or any other device is a good thing to use without experience. I believe we shall be able to suture far more rapidly with Dr. Grant's device than without it.

Dr. W. E. B. Davis, of Birmingham, Ala.—A large number of operators have abolished mechanical devices in doing anastomosis. My brother (Dr. John D. S. Davis), who devised a catgut plate and mat, now prefers not to use them. Of course, there are many cases in which the device of Dr. Grant cannot be used—those in which you wish to do an anastomosis without resection of the bowel. Where you make a resection it is an ingenious device, and one that will facilitate the surgeon's work and enable him to do the operation very quickly. The plan I have been working with has been not to use a plate, but to take interrupted sutures, then outside sutures, after having taken the through-and-through sutures. These stitches require time, and where you make a resection of the bowel this instrument of Dr. Grant will save much of that time. In this connection it will be appropriate for me to say that I have conducted a series of experiments in the effort to do away with mechanical devices, with an end-to-end operation, by splitting the bowel. While the opera-

tion promised to be successful, the strain on the circulation was too great, and I wish now to condemn it. There is no question in my mind but that it is a very dangerous procedure.

Dr. G. Frank Lydston, of Chicago.—While I am not practically familiar, to any great extent, with any of the devices for intestinal anastomosis, I desire to call your attention to a recent device originated by my friend, Dr. J. B. Murphy, of Chicago, which he calls his "anastomosis button," and which was used and beautifully illustrated upon a dog at a recent meeting of the Mississippi Valley Medical Association, held in Cincinnati. As far as the time-saving element of mechanical devices is concerned, I presume every abdominal surgeon recognizes its value in operative work. In this respect the device of Dr. Murphy is superior to anything that has yet been devised. With it a cholecysto-enterostomy can be done in from seven to twelve minutes.



Appearance of button with and without spring-cup attachment.

The anastomosis button. The buttons are made in three sizes. A button consists of two small circular bowls; size No. 2 measures as follows: Diameter, 23 mm.; depth, 8 mm. There is "sweated" into a circular opening, 12 mm. in diameter, at the bottom of one bowl, a cylinder 15 mm. in length, with female screw thread on its entire inner surface. The cylinder extends perpendicularly from bottom of bowl. There is an opening in the male bowl in which is "sweated" a similar and smaller cylinder of a size to easily slip into the female cylinder. There are two brass springs soldered on either side of the inner surface of the lower end of the male cylinder, which extend almost to the top, where small points of them protrude through openings in the cylinder; these points are designed to catch the screw-thread when the male cylinder is pressed into the female cylinder,

and thus hold the bowls together at any point desired. To separate them again they are simply unscrewed. A small brass ring, with a thin though not cutting edge, to which is attached a wire spring, is placed in the male bowl and retained in position, projecting one-eighth of an inch above the edge of the bowl. This is held up by the wire spring, and is there for the purpose of keeping up continuous pressure until the entire tissue between the edges of the bowls is cut off. This spring attachment is absolutely necessary only when the stomach is operated on. There are four openings, 5 mm. in diameter, in the side of each bowl, for the purpose of drainage. By this, it will be seen, we have two hemispherical bodies held together by invaginating cylinders. These hemispheres of the button are inserted in slits or ends of the viscera to be operated on. A running thread is placed around the slit in the viscus, so that when it is tied it will draw the cut edges within the clasp bowl. A similar running thread is applied to the slit in the viscus into which the other half of the button is inserted, and the bowls are then pressed together. The pressure atrophy at the edge of the bowl is produced by the brass ring supported by the wire spring. The opening left after the button has liberated itself is the size of the button.

This differs from all other previous devices in the following particulars or combinations thereof: 1, It retains its position automatically; 2, it is entirely independent of sutures; 3, it produces a pressure atrophy and adhesion of surfaces at the line of atrophy; 4, it insures a perfect apposition of surfaces without the danger of displacement; 5, it is applicable to the lateral as well as to the end-to-end approximation; 6, it produces a linear cicatrix and thus insures a minimum of contraction; and 7, in the extreme simplicity of its technique, which makes it a specially safe instrument in the hands of the everyday practitioner as well as the more dexterous specialist.

A PLEA FOR MORE RAPID SURGICAL WORK.

By Ap Morgan Vance, M.D.,
Louisville, Ky.

In the days before the discovery of anæsthetics surgeons were noted just in proportion to the rapidity and dexterity of their work. In our days, however, I can safely say that a great number of our brethren of the knife pay little attention to the time consumed in operation, or to that nicety of manipulation and dexterous use of instruments that our forefathers prided themselves upon.

I write this very short paper as a plea for a more proper appreciation of the value of rapid and, at the same time, perfect work. I fear that many fail to heed the dangers of prolonged anæsthesia, and often consume a great deal too much time in operations, much to the detriment of the patient. I am sure I have seen this occur on numerous occasions in the work of our most distinguished surgeons, and have seen deaths occur which would not have taken place if much unnecessary time had not been wasted. The habit of starting the anæsthetic before all preparations are completed and everything ready for immediate work at the word from the anæsthetist is, in my opinion, very reprehensible.

I recently saw a surgeon chloroform a patient himself in a remote part of a large hospital, move her to the operating-room; then, having given the anæsthetic into the charge of another, leisurely prepare himself for the abdominal operation proposed.

Another practice to be condemned is that of anæsthetizing a second patient and then waiting in the ante-room for the first

operation to be finished. There are various habits that some surgeons fall into which are calculated to waste time: that of discussing with assistant or bystander whether or not such a step is best, and hesitating whether to cut this or that. He should have planned his work as far as possible beforehand, and go through to the finish, keeping himself well in hand and remembering general surgical laws as his guide. The same can be said of the operator lecturing to those present, or talking on other subjects during an operation.

I have long since been convinced that far too little attention is paid to the manual training of the surgeon, and I know how painful it is to see an awkward and unskilful manipulator doing an operation. I suppose this is partly due to the fact that many men drift into surgery, and have never had that primary education necessary to skilful and rapid use of instruments.

I suppose in many cases that much time is lost on account of the lack of confidence on the part of the operator, and in needless dissection. Familiarity with different tissues should be carefully cultivated, and the feeling in one's mind encouraged that you are as well able to do the operation as anybody else. I do not know of a more painful sight than to see an operator who has so little confidence in himself that he is constantly hesitating and waiting for encouragement from those assisting.

I was greatly pleased recently in speaking to Dr. Howard A. Kelly, to find he was an advocate of rapid work, and I find in a report of fifty cases of abdominal section reported by him recently in the *Johns Hopkins Hospital Reports*, that the average time was twenty-one minutes. If two of these cases, which were unusual, were left out, the average would be eighteen minutes, which shows that he practises what he preaches.

I hope to see the day when these slow workers will have to learn to use their hands, or be left far behind in surgical progress.

DISCUSSION.

Dr. Arch Dixon, of Henderson, Ky.—In regard to Dr. Vance's paper, I will say that gentlemen who live in cities and operate in hospitals and infirmaries, and only have two or three surgeons around them, can operate with rapidity, but in small towns, where you have to invite every doctor in the town as a matter of courtesy, and have to stop to get the opinion of each one, your operation is bound to be delayed. Surgeons in hospitals can operate without delay, and without interruption, at such and such an hour; but it is different with surgeons who live outside of large cities. Every practitioner who is called in to see you operate in the country is expected to be asked his opinion, when he really may not know anything about the case.

Dr. Rufus B. Hall, of Cincinnati.—I rise simply to differ from the gentleman who has just taken his seat (Dr. Dixon). In every surgical operation it is a recognized fact that one man only is responsible, and that is the operator. This being the case we have no right to ask anyone out of courtesy or a bystander's opinion as to what should be done in any given case at a surgical operation. Our duty to the patient and to ourselves is to make an operation that, in our judgment, is best for the patient, and it should be as rapidly done as possible for the welfare of the patient. If a man gets offended because you do not ask him his opinion in the case, that ought not to cut any figure at all with the operator.

Dr. George A. Baxter, of Chattanooga, Tenn.—I think the most important point that Dr. Vance brought out in his paper is the careless use of anæsthetics. It is a fact that a great many practitioners fail to regard the amount of anæsthetics they give to patients. I will warrant that in Dr. Kelly's investigations and operations in the Johns Hopkins Hospital the amount of the anæsthetic and the time of giving it are noted, and that these have a considerable bearing upon the future progress of the case. Dr. Joseph Price has repeatedly referred to what he terms the "water-logging" of a patient with anæsthetics, "battening" the patient down, as he calls it. There is no question but what the dangers of chloroform will be very much lessened, and perhaps

chloroform very much more used than it is at present, if greater care were taken in regard to the amount. The matter of some operators in giving an anæsthetic in one room, *then* making proper preparations for an abdominal section, affects the after-progress of the case and jeopardizes the success of it.

Dr. Edwin Ricketts, of Cincinnati.—There is one point that was not brought out in the paper or discussion. I refer to the manipulation of instruments. If we are going to do surgical work we must pay more attention to the manipulation of saws, chisels, gouges, etc. Much time would be gained in surgical procedures if more attention was given to this particular subject of manipulation of instruments. The surgeon, perhaps, picks up a knife to make an incision, and when the incision is about half completed he possibly lays it down, asks some question in regard to the case, and then completes the incision. This is very frequently done. There is much to be gained not only in the manipulation of instruments, but, as has been suggested by the last speaker, in the length of time that the patient is to be anæsthetized. The fact of putting a patient under an anæsthetic and letting him or her be in an adjoining room until the operation on another patient is completed is a procedure that should be condemned by every man doing surgery.

In regard to the preparation of patients in abdominal work, everything should *be on time.* The assistant should know his place and his work, and should be able to do more than that. He should be by the side of the operator and anticipate his wants as to the instruments needed. Dr. Vance's paper is a short one. It is to the point. It is what we want.

SURGERY OF THE URETERS, WITH REPORT OF CASES.

By Charles A. L. Reed, M.D.,
Cincinnati.

Surgery of the ureter is one of the developmental subjects of abdominal surgery. These out-of-the-way conduits, exercising functions that are vital in character, are liable to diseased conditions which baffle the resources of the diagnostician and tax the ingenuity of the operator. It is important for us to call into brief review the means at our disposal for first detecting and next treating disease involving these aqueducts.

For purposes of diagnosis the physical means at our disposal may be briefly summarized as follows: 1. Exploration of the lower end of the ureters by digital examination (*a*) through the vagina, (*b*) the rectum, (*c*) the bladder; 2. Exploration of the lower end of the ureters by the sound passed through the urethra and bladder into the ureters; 3. Exploration of the central portion of the ureters by abdomino-lumbar palpation—an expedient of practical value only in cases of extreme ureteral distention occurring in very thin subjects; 4. Exploration of the upper end of the ureters by exploratory nephrotomy. Each of these several expedients might be written of in extreme amplification, but I feel that to do so is uncalled for in this presence. It may be proper, however, to add that since the catheretization of the ureters has been popularized in this country, chiefly through Dr. Kelly, and since the technique of the procedure has become understood by those who have studied it, the diagnosis of disease within and surrounding these tubes is vastly more common. The digital

exploration of that portion of the ureters lying within easy reach from the vagina or rectum is readily practised by those who have carefully studied the anatomy of the parts. Digital exploration through the urethra and bladder is an easy expedient so far as the surgeon is concerned, and often leads to the elucidation of important pathological facts, but I am forced to believe that it is not without danger to the patient. I have been forced into this belief by one case of incontinence lasting for nearly a year, and by two cases of weakened power of retention, one of which is now of quite two years' standing. I feel, therefore, that the practice of intra-vesical digital exploration ought not to be practiced except as the last resort. The endoscope has not been mentioned in this connection, for the reason that its use implies the extreme dilatation of the urethra, and, even when the latter is dilated, the endoscope is often unsatisfactory in failing to reveal the ureteral orifice, and when it does reveal the ureteral orifice it it still unsatisfactory, as the information that it thus imparts is generally of little diagnostic or other value. There is another means of ureteral exploration that I have not mentioned in the foregoing summary, for the reason that I desire to consider it separately for the sake of emphasis, viz., that by *exploratory incision of the vesico-vaginal septum.* When the vast importance of correct diagnosis in ureteral disease is taken in consideration, and when thought is given to the fact that incision of the septum, as suggested, always heals by first intention and without detriment to the patient, *i. e.*, when done in accordance with the canons of modern surgery, the procedure is at once justified, and must take its rank as a proper diagnostic expedient. This view is fortified by the reflection that the incision at once places many of the diseased conditions under command for a completed operation. Abdominal section for diagnosis of ureteral conditions, notably in cases of suspected calculus, is entirely justifiable.

The consideration which I have given to the various measures which may be employed in the detection of ureteral disease, and particularly the emphasis which I have given to

vesical, lumbar, and abdominal incisions for diagnostic purposes, render it unnecessary for me to give a *résumé* of the general surgical resources employed in the treatment of these cases for any of these incisions, singly or in combination, when primarily explorations may be easily converted into a completed operation whenever indications may prompt. My only motive in passing these general observations under cursory review is to pave the way to the discussion of a few cases to which I invite attention.

Case I. *Peri-ureteritis; stricture; kolpo-cysto-ureterotomy; recovery.* Mrs. M. was sent to me by my friend, Dr. Van Dyke, of Mason, O. She had had a child seven years previously, and a miscarriage three years ago. She convalesced slowly from her labor, and was in bed several weeks following her miscarriage. Her final convalescence from the latter occurred only after the free discharge of pus from the vagina. After convalescence from this attack she began having pain in the right loin, extending well down into the right iliac region. After a few months the sediment in the urine seemed to be purulent in character. When she came to me, in September, 1892, the urine abounded in leucocytes, epithelial cells, and oxalates, but no tube casts or other kidney débris could be discovered. On digital examination my attention was at once arrested by the expression of pain whenever I touched a point just in front and a little to the right of the uterus. It required no dexterity whatever to clearly outline a ureter in this position, for the reason that this canal at its lower extremity was enlarged to a size larger than a lead pencil, was very tense, and extremely sensitive. She came into the hospital, when, under anæsthesia, I made a careful exploration of the pelvis. The ovaries were of normal size and mobility, while the Fallopian tubes, both of which could be distinctly palpated, were unenlarged. At the same sitting I introduced through the urethra the smallest-sized linen catheter mounted on a stilet, and bent to facilitate easy introduction into the ureter. The index finger of the left hand in the vagina was all that was necessary for a guide. I, however, found it impossible to introduce the catheter into the ureter, at the orifice of which a small nodular body could be easily distinguished. I at once made a free incision

through the vesico-vaginal septum, and readily brought the ureteral orifices alternately into view. On the left was a normal orifice through which the urine escaped in normal rhythmical jets; on the right there was a gradual ooze from a point just back of a small mass of granulation tissue. A catheter was inserted in the healthy ureter and left long enough to show that the urine escaping therefrom was entirely free from appearances of pus or other pathological features. The small mass of soft granular tissue was now curetted away from the orifice of the right ureter into which the catheter was now introduced, only, however with a force that was appreciable. This was immediately followed by the escape of urine containing pus and epithelial cells, with a few particles of sand. Catheters were now inserted and withdrawn in the ascending scale until a No. 8 was used. The ureter was then flushed with sterilized water, after which the catheter was passed to the pelvis of the kidney, and permitted to remain for a few minutes, during which time the stitches for reclosure of the vesico-vaginal incision were inserted. The urine that escaped during this time was subjected to microscopical examination and found to contain no pathological elements. The patient made a prompt recovery, and was at her home within two weeks. There is some remaining thickening and sensitiveness of the ureter, and I have informed her that there is liable to be slight recontraction, to prevent which I have advised her to submit to redilatation after the lapse of a couple of months, or sooner if indicated.[1] This can, of course, now be done through the urethra and bladder, without dilatation of the former, now that the obstruction in the form of the granular excrescence is removed from the mouth of the ureter.

Peri-ureteritis has recently come to be a recognized form of disease. In the present instance its pathological antecedents are of great interest. The history of the case embraced a clear record of pelvic suppuration. As is frequently true with suppurations within the pelvis, occurring as the result of trauma to the endocervium, incident to the induction of abortion, pus evidently formed in this case within and at the ex-

[1] This patient is now (April 5, 1893) entirely well, with no remaining enlargement, induration, or tenderness of the previously involved ureter.

pense of the cellular tissue above the right fornix of the vagina. My inference that such was the condition in this instance is forced upon me by the fact first, that these etiological factors existed; next, that infection to a wound in the anterior of the cervix naturally followed the lymph channels into the cellular tissue, where it generally induces suppuration; next, that both physical exploration and the subjective symptoms of the case exclude the appendages from present participation in the disease, a fact which would not be true had pus ever involved the Fallopian tubes; and finally, because of the sequent changes at the lower extremity of the ureter. These changes were doubtless of contraction of the thickened walls of the canal by the organization of inflammatory exudate. The contraction resulted in the retention of urine within the ureter and kidney, and it did more. Either the endo-ureteritis, which occurred as the result of extension of the original inflammatory process, or by induction of the same by structural interference of the nutrition of the part, the epithelium at the valve-like orifice of the ureter was destroyed, and inflammatory exfoliation took place. The removal of this granular mass with dilatation of the canal, and consequent partial restoration of the normal circulation, will doubtless re-establish the normal status of the canal.

Case II. *Cicatricial stricture of an excised ureter; hydronephrosis; nephrectomy; recovery.* Mrs. T., aged forty-one years, was sent to me by my friend, Dr. Armstrong, of Cincinnati, in May, 1890, for vaginal hysterectomy for cancer, in the performance of which the left ureter was caught in the clamp, resulting in a ureteral fistula. I advised that she submit to the annoyance until graver symptoms should prompt interference. These symptoms occurred a year later, and consisted in marked diminution in the quantity of the dribbled urine, the development of pain in the left kidney and down the ureter—in short she had hydro-uretero-nephrosis. An effort to dilate the lower end of the ureter was fruitless, and nephrectomy was done by lumbar incision, from which she promptly recovered. The ureter was included in the pedicle. She has since been without trouble other than that of a severe rectocele, for which I operated within the last few weeks.

I mention this case in the briefest possible outline for the purpose of indicating certain salient points which I wish to have utilized as texts for further discussion. In this case I had to deal with an excised ureter. In the end of that ureter was a deposit of cicatricial tissue, which was itself imbedded in, or, for that matter, was a part of the more extensive cicatricial tissue which performed the important task of closing the upper end of the vagina. The necessary result of normal reparative changes in this territory must have been the final occlusion of the lower end of the ureter. To have dilated it would only have been to give it an opportunity to recontract, Before removal of the kidney was decided upon in this case the question of switching the ureter into the bladder or rectum by plastic process was debated, but a negative conclusion was reached by the reflection that the cicatricial ring would continue to contract whether located in the bladder or the rectum or elsewhere, and that necessity for removal of the kidney would not thereby be even deferred, or if deferred, only for a short time.

I trust that the Fellows who have had time for research, or who may have had experience, will favor us with the after-history of cases in which ureteral implantation has been practiced.

Case III. *Pyonephrosis; nephrectomy; remaining ureteral disease.* Mr. B. was under the care of my friend, Dr. Scheibenzuber, of Dayton, Ohio, who diagnosed pyonephrosis of the left side, for which I did a lumbar nephrectomy November 16, 1891. The previous history of the case ran through several years, during which time pus had been constantly present in the urine. Pain located in the left kidney was the most conspicuous symptom. On palpation the kidney on that side could be outlined as distinctly enlarged. The pain complained of was of the constant sort, and never colicky in character, and was located entirely in the left kidney. Careful microscopical examination of the urine by Dr. Scheibenzuber revealed the presence of kidney débris in great quantity. On coming down to the capsule, at the time of operation the latter was fluctuating, and on opening it there

escaped a considerable quantity of fluid which looked like bloody urine. The capsule was found very generally detached from the kidney, and both were removed without extreme difficulty. The kidney was enlarged and somewhat dilated; the pelvis was full of pus; the apices of the pyramids were largely destroyed, and there were several centres of suppuration in its parenchyma. The patient made an entirely satisfactory recovery so far as the operation itself was concerned.

There were some features of the case, however, that were and are yet embarrassing. To begin with, he, of course, had pain in the other kidney during the few months that it was undergoing compensatory development, a fact which would not have been confusing to the patient had it not been that the discharge of pus continued and yet continues. These two facts were construed by the patient as signifying that if the wrong kidney had not been removed there was at least disease in the remaining one, to which, however, no symptoms had been referred prior to the operation. It was at first supposed that the purulent urine depended upon a purulent cystitis for its continuance, but careful flushing of the bladder by boric acid, nitrate of silver, and other solutions failed utterly to relieve it. Time, however, has done something to clear up the case. The pain in the remaining kidney has disappeared, and that upon the affected side has become located midway between the site of the former kidney and the bladder. On the occasion of a visit to my office a month ago a careful microscopical examination of the urine was made by Professor Kebler. There was pus in abundance, normal epithelial elements, but no kidney débris whatever. This state of affairs tends to confirm the diagnosis made by the patient himself—a very intelligent man—to the effect that the fountain head of the pus is located in the ureter on the right side. Whether the pus-manufacturing process is there kept up by the presence of a calculus or a stricture cannot be determined without an exploratory abdominal incision, which has been agreed to by the patient.

One cannot, of course, resist the temptation to subject to

critical review all steps of an operation that is followed by unsatisfactory result. I have catechised myself severely as follows: Was a diagnosis of ureteral disease practicable before the operation? Would it not have been practicable to have explored the ureter by sound, by incision through the kidney, before removing the organ itself? Would it not have been better to have done the operation by abdominal incision? Would it not have been advisable to have done the operation by double incision, *i. e.*, one in the loin and one in the belly? In reply to these several inquiries I have been forced to conclude that it would have been possible to have located, more definitely than was done, disease within the ureter, had either subjective or physical symptoms pointed in that direction. As it was, every indication pointed to the kidney as the sole seat of disease. It would have been practicable to have sounded the ureter through the kidney, but again comes the reason that there was no indication for doing so, particularly as exploration of the kidney had already shown conditions sufficient to account for all the symptoms in the case. The same reasons will answer the question as to the expediency of lumbo-abdominal incision—a method which has been practised successfully by Hale and others for ureteral calculus. Even if I had been prompted by the revelations of the exploratory cut in the back to have made a counter-incision, and introduced my hand through the abdomen, I would, in all probability, have declined to have done so, because of the the extremely weak condition of the patient at the time.

I ask the pardon of the Association for bringing these desultory reports into the proceedings, but I am prompted to do so by the hope that the cases here recorded will elicit a discussion more heavily freighted with fruit than is this purely impromptu paper.

DISCUSSION.

Dr. Arch. Dixon, of Henderson, Ky., reported the following case in connection with Dr. Reed's paper:

On the 26th day of December, 1891, Jim C——, in an alterca-

tion, received a knife wound at a point midway between the lower border of the ribs and the crest of the ilium in the axillary line of the right side. The wound was about two and a half inches long and penetrated the cavity of the abdomen. A physician was called, and two stitches were taken in the centre of the cut, the ends being left open, it was said, for drainage. On the fourth day after the infliction of the wound I was called to see the case, and found the patient with a temperature of 103½°, pulse 115, tongue dry and coated. The wound was suppurating, and at either end there was a protrusion of the omentum. The stitches were removed, the omentum drawn out and scissored off; irrigation with sterilized water and a tamponade of iodoform gauze completed the dressing. Improvement was immediate and pronounced, and in a short time the wound was closed and the case dismissed. Three days after dismissal the patient came to my office complaining of pain and an enlargement over the site of the cicatrix. Examination revealed a distinct enlargement with fluctuation. An aspirating needle, which had been thoroughly sterilized by passing it through the flame of an alcohol lamp, was introduced, and about six ounces of purulent fluid, having the odor of urine, was drawn off. The patient was instructed to call again the next day. An examination of the fluid drawn off was made, and urine was found to be present. On presenting himself the following day the cavity was again found to be tense and filled with fluid. An incision was made at the seat of the former wound. As this was the *locus minoris resistentiæ*, about six ounces of urine flowed out. A drainage-tube was inserted after irrigating the abscess cavity with sublimate water and peroxide of hydrogen. This treatment was kept up for some time, until finally the pus disappeared, but the discharge of urine was unabated. Nephrectomy was suggested as the only means of relief, but was rejected. I saw no more of the case until about August 1st, when I was called and found there had been a return of the pus, with a marked decrease of the urinary flow. During all this time the patient had worn a small rubber tube, which fitted closely into the fistulous opening. The tube was kept stopped with a cork, which was removed at intervals of four or five hours to permit the passage of the urine which collected in the cavity. A treatment of irrigation with sublimate solution and hydrogen peroxide was again prescribed, but with little or

no improvement. On August 29th, Dr. Arch. Dixon, Jr., did an exploratory operation with the view of making either nephrotomy or nephrectomy, as the case demanded.

Peri-nephritic abscess is in most cases secondary to abscess of the kidney itself. Greig Smith says it occurs also as a primary idiopathic disease, and also as a consequence of urinary extravasation, or renal fistula. Abscess of the kidney itself was suspected in this case, and for this reason operation was advised. The oblique lumbar incision, as recommended by Morris, was not followed, in so far as there was already a fistulous opening leading down to the kidney, but the incision was made through the fistula downward and forward toward the crest of the ilium. It was not found necessary to divide the deep lumbar aponeurosis, as the abscess cavity extended quite down to and below the kidney. Digital examination was made, and the kidney could be felt firm and seemingly without disease, so that it was thought best to do neither nephrotomy nor nephrectomy. The pus cavity was thoroughly washed out with sublimate solution, and was then curetted and tamponed with iodoform gauze. The sublimate irrigation and tamponade of iodoform gauze was kept up daily for some time, with marked improvement in the condition of the patient, whose appetite increased wonderfully with a pronounced gain in weight. There was little or no return of the purulent discharge for several days, but it did return, as also did the discharge of urine. Iodine and etherized iodoform were a number of times injected into the cavity, but with only a temporary benefit. The fistulous opening still remains, with a drainage-tube inserted through which flows daily several ounces of pus and urine.

Now, what is to be done in this case? Greig Smith says: "In all cases of suppuration in and around the kidney incision with evacuation of pus and drainage of the abscess sac are indicated." Contra-indications in such cases are: *First*, such a condition of exhaustion as would negative any serious surgical exploit; and, *secondly*, a diseased condition of the opposite kidney. Whenever operation for abscess is feasible, nephrotomy ought to be the first operation. The prime object is evacuation of the pus; secondary objects are diagnosis of the actual state of affairs and determination of the chances which nephrotomy provides toward cure, and

preparation of the kidney and the patient for the major operation of nephrectomy when incision cannot be expected to be curative. Nephrectomy performed in the first place as an operation for suppurative lesions of the kidney is not so successful as nephrectomy performed as an operation following nephrotomy and drainage. The patient gains strength after the evacuation of an abscess, and the kidney decreases in size, while the vascularity of the organ and the density of the adhesions become less marked after drainage." While nephrotomy was not done in this case, yet the abscess sac was opened up and thoroughly drained, and as a consequence the patient has gained strength and is in a much better condition physically than before,

I beg leave to show you the patient and to ask what shall be done for him? My own opinion is that the case demands nephrectomy.

Dr. Howard A. Kelly, of Baltimore.—I wish to draw the attention of the Association to my ureteral catheter and ureteral sounds, as well as to draw your attention to an article published in the *American Journal of Obstetrics*, vol. xxv., No. 6, 1892. I give three photographs of the catheters introduced, showing catheters in both ureters simultaneously, draining off the urine directly from both kidneys drop by drop.

Description of ureteral catheter. The catheter thus made is a slender metal tube, thirty centimetres in length and two millimetres in diameter. At the end which is introduced into the ureter it is slightly curved for two centimetres, and terminates in an olive-shaped point 1.5 millimetres in diameter. Any further diminution of the size of this point renders it liable to pierce the bladder in the attempt to catheterize the ureter, while if it is larger it is difficult to introduce into the ureteral opening.

I found that the long slit of Pawlik's cathether, which lets the urine into the catheter, would frequently catch and cut the mucous membrane of the urethra as it was being carried into the bladder; I have replaced this in my own instrument by several perforations in a little gutter countersunk on the concave side of the shaft near the point of the instrument. The opposite end of the catheter, at the handle, is provided with a lip curving downward to facilitate the discharge and collection of the urine in a finely graduated tube. During the introduction of the catheter this end of the tube is plugged with a short metal rod, otherwise the

Kelly's ureteral catheter.

urine would continually escape from the bladder while the orifice of the ureter was being sought. This little rod is attached by a fine chain to the catheter to prevent its being lost. I have placed a fixed metal handle four centimetres from the end of the instrument, six centimetres in circumference, and flattened on the side toward which the point is directed. This enables one to conveniently hold and direct the instrument in its introduction, and is better than the split movable wooden handle previously in use.

The catheter thus constructed is altogether a convenient instrument, and its introduction one of the most delicate, pleasing gynecological operations. I often thus introduce two catheters at the same time—one into each ureter—when, by hanging a little test-tube on the end of each, urine is simultaneously collected from both kidneys.

I also show a ureteral sound (exhibiting), one of a series which I am using in the treatment of stricture of the ureter. In the *Johns Hopkins Hospital Reports* on Gynecology is a case where I incised a ureter to get at a stricture which could not be treated through the urethra and the bladder. I first incised the vault of the vagina, then caught and pulled down the ureter, split it open, and stitched it to the vaginal wall, and treated the ureteral stricture in the posterior part of the pelvis directly through this vaginal opening until it was sufficiently dilated, when I closed the vaginal opening without difficulty. I have at the present moment two ureteral cases in the Johns Hopkins Hospital illustrating very nicely the utility of catheterization. The first patient suffered for months from hematuria; she had no pain, and no dyscrasia to account for it. Where did the blood come from? On catheterizing both ureters at the same time clear urine came through the right ureter and thick bloody urine came from the left.

Another case catheterized but a few days ago had a cystitis with enlarged ureters. On catheterizing the right ureter the urine dribbled in the normal manner, drop by drop, at intervals of a few seconds. On catheterizing the left ureter, nothing at all came through the catheter until I introduced my finger into the rectum, and pushed the catheter a little higher up when it passed through a stricture, and within a minute and a half nine cubic centimetres of urine were discharged through the catheter; a

simple calculation showed that if the urine was actually being secreted by the kidney with that rapidity the daily secretion would amount to many gallons. It was clear, therefore, that we were dealing with an accumulation of urine above a stricture—*a hydro-ureter with stricture.*

Dr. Edwin Ricketts, of Cincinnati.—The first case of abdominal nephrectomy I did was in a man suffering from peritonitis. I was at a loss to know what was the cause of peritonitis. Upon opening the abdomen, quite a quantity of fluid escaped, and I made an examination of the left kidney. About three inches below the kidney I found that the ureter had ruptured. I removed the kidney and ligated it *en masse,* and at the point of rupture I took out three good-sized stones. When I found this rupture I was satisfied as to the cause of peritonitis. I lost the case. I have since done two lumbar nephrectomies in females. Almost three years have elapsed since the removal of the kidneys. One of the patients is now teaching school, it being her second year of work.

In regard to the case presented by Dr. Dixon, I agree with Dr. Kelly that a nephrectomy seems advisable. No doubt Dr. Dixon will encounter a good deal of difficulty in operating, on account of the dense adhesions which the kidney has around it.

Dr. Hunter McGuire, of Richmond, Va.—I feel gratified at the work that is being done in the direction of surgery of the ureters. The ureter until lately has been almost an unexplored portion of the human body. We are greatly indebted to the gentlemen who have worked in this direction for some light upon diseases of these tubes. Usually the diagnosis of changes in them is exceedingly difficult. I congratulate Dr. Reed upon being able by palpation to detect some change in the ureter on one side in his patient. I know it has been suggested, and I have frequently tried it, but I never have succeeded in getting any information in that way. If the ureter was a tube, like the bowel, full of gas, you would get a resonant percussion there, and outline it without much trouble; or if it was a tube full of water you would get a flat, dull, percussion sound. As it is an empty flaccid tube, it is difficult, with the finger in the rectum or vagina, and one on the outside of the body, to detect trouble. I have tried it over and over again and failed. I will try again, since

Dr. Reed said he was able to find it. Usually disease of the ureter begins with some obstruction to the function of urination. It may be a stone, prostatic enlargement, or the existence of a tumor. Last and most rare of all, it may be due to atony of the bladder, a rarer condition in my experience than is usually supposed. Whenever there is obstruction to the function of micturition there is a strain put upon the ureters. You may compare the bladder to a large bag full of water; you have hydrostatic pressure which presses in all directions. It may open the mouths of the ureters, and after a while distends them. If this is the case with the normal bladder, you see how much more pressure is exerted when there is prostatic obstruction or stricture; and how much more frequently pressure is exerted by the bladder to empty itself than in its ordinary condition. With the increased frequency of urination, this produces, after a while, dilatation of the ureter, dilatation of the pelvis of the kidney, consequent inflammatory changes, and presently pyelonephritis, or what we used to call "surgical kidney." This is the bugbear of most genito-urinary surgeons. Most of us here have had cases of enlarged prostate with retention of urine. We have passed in a catheter to empty the bladder, and have been surprised in a few hours to find the patient has a chill or fever and is in a profuse sweat. Presently he has all the symptoms of urinary poisoning, and dies. This has happened to many us. The man had, probably before the catheter was used, surgical kidney. I speak of this because there is no way to diagnose those conditions. There are no signs or symptoms by which we can tell it, and yet if it does exist, as it always does in the last stage of obstruction to the function of urination, the most trivial operation will kill. Even the introduction of a catheter will kill, and the lithotrite or lithotomy is fatal. I was much surprised when Dr. Brown read his admirable paper yesterday, giving a biographical sketch of the late Dr. Dudley, to hear him say that Dr. Dudley had performed 225 operations for stone with only 3 deaths. It was a most remarkable statement. All of his operations were done by cutting through the peritoneum. He operated, remember, before the days of antiseptic surgery. I learned afterward from a fellow pupil of Dr. Brown's and an old student of Dr. Dudley's, that he rejected a number of cases. He kept them under obser-

vation sometimes for months, and frequently at the end of eight or ten months declined to cut for stone, and this gave me some explanation of the wonderful success of that wonderful man. In the run of cases nowadays, I do not believe any man can take as they come 225 cases of stone and operate, and have anything like such a record as Dr. Dudley had. It is chiefly because of the existence of a curious condition of the kidney which we have no means of determining before death. Some young man in the profession will immortalize himself if he will find out how to foretell the existence of this condition. In these cases you find the urine normal, the specific gravity about 1018 or 1020; you find it normally acid; you find no more albumin in it than you suppose would exist in consequence of pus and blood which accompany stone, prostatic enlargement, or some obstruction. There is so far no way that we have been able to tell. Lately it has been suggested that there must be in kidneys crippled in this way a deficiency of urea. A crippled kidney or a pair of partially diseased kidneys will eliminate urea, just as partially diseased lungs will eliminate carbonic acid gas and take oxygen enough from the air necessary to the life of the individual. It would be a great triumph in surgery if some one would go to work and tell us when a man has pyelonephritis before we operate, and not, after the patient is dead, make an examination and then find it.

Dr. Reed says the best way to get at the ureters to find out if there is stricture or obstruction, is through the vesico-vaginal septum. He confines his cases exclusively to women. Even in women I think the best way to do it is by a suprapubic cystotomy. Suprapubic cystotomy is almost as simple to those who have done it as opening an abscess. You go through a structure which has no vessel or nerve that is dignified by a name. I have gone into the human bladder above the pubes, and not lose a teaspoonful of blood, and cut through no structure whatever that you would be afraid to cut. You see then right down into the bladder with a little electric light as big as a woman's thimble or pea light. You carry it down into the bladder if you chose, and you have almost as complete inspection there as if it were on the outside of the body. If you have no electric light, carry two fingers into the rectum, push the base of the bladder up; you can push it up

into the suprapubic hole you have made in the skin; you can then carry a sound into the ureter, and find out if there is a stricture or stone, by a method which is infinitely better than by going through the vesico-vaginal septum.

Dr. Joseph Eastman, of Indianapolis Ind.—Just one point in connection with the case referred to by Dr. Reed. In forty total extirpations for cancer of the uterus I have had two cases of utero-vaginal fistula. One was caused from simply removing some necrotic tissue when I had a discharge of urine into the vagina. The second case occurred before I learned how to cut, first through between the bladder and the uterus, then between the uterus and the rectum, severing the mucous membrane, surrounding the broad ligament with a strong ligature, tying it, reducing the broad ligament completely to a rounded mass; then, with a staff grooved on its concave surface, I secure the broad ligament, as I have it represented here (illustrating on the board), constricted to a round mass. I may then use the ligature or clamp, transfixing the broad ligament and securing it in the way you see here. The point I make is this: If the broad ligament is rounded into a given mass, we have complete sero-serous approximation above it, and each stump is in the angle of the orifice we have left. If we have in any way injured the ureter, which it is almost impossible to do (if we observe this precaution), we can tell which side the urine is trickling through. In one case I passed a sound into the bladder, bringing it out near where the urine was being emptied, cutting on to point of sound, then stitched off a little recess of the vagina near cicatrix, allowing the urinary tract to extend from the orifice of the ureter in the bladder. I did not wait quite as long as I should have done. I had breaking down of one corner of the suture line, which would not have occurred had I waited a few weeks.

To recapitulate. The rounding of the broad ligaments, so that all severed tissue is within the vagina, enables us to get complete sero-serous approximation, and is a decided advantage. We can dilate the urethral orifice, and there is less difficulty in finding where the orifice is than where the ligaments are not first rounded.

Dr. Reed.—How long is it since you operated on that case?

Dr. Eastman.—About a year.

Dr. Bedford Brown, of Alexandria, Va.—I am going to describe the indications of some cases of gonorrhœal ureteritis which have come under my observation. These cases were described in a paper read before this Association at the Richmond meeting last November, entitled "Remarks on Systemic Infection from Gonorrhœal Poisoning." (See vol. iv. of *Transactions*, 1891, p. 38.) I simply wish to dwell upon the signs of differentiation in these cases. The gonorrhœal inflammation had extended from the prostate to the bladder, causing cystitis, and finally ureteritis, pyelonephrosis and nephritis. The pain in the ureter extended in the line of that canal up in the direction of the kidney. In nephritic colic, the passage of a gravel, the pain descends invariably from the kidneys toward the bladder, but in ureteritis the pain ascends from the bladder in the direction of the kidneys. The line of the ureter can be traced by palpation; the constant tenderness on pressure can be discovered almost the entire line of the ureter, and the patient has a tendency to lean on the side in the direction of the inflamed ureter. I simply desire to call the attention of the Association to these indications in connection with the cases cited by Dr. Reed.

Ureteritis is always in gonorrhœal infection consecutive to specific cystitis, and never precedes it. In specific cystitis, when pain and tenderness extend up the line of the ureter, it indicates ureteritis.

Dr. C. A. L. Reed.—I feel grateful for the discussion on my paper, and I feel especially obliged to Dr. Dixon for having brought here to-day a case of such extreme interest as that which he has exhibited in our presence. With regard to Dr. Dixon's case, I feel that he and Dr. Dixon, Jr., have pursued a conservative course in its management. Looking to the future, I would, if the case were mine, begin by making an anterior incision, introducing my hand into the abdominal cavity, putting the patient on the left side, and then making such incision in the loin as might be necessary to relieve the existing condition. I believe, if changes have taken place incident to inflammatory adhesions, and so on, that this expedient is necessary to enable the operator to deal with the condition satisfactorily. I believe when Dr. Dixon comes to perform this operation that he will find the conditions which he encounters a repudiation of the position he has

taken, that nephrectomy is an easier operation after the previous performance of nephrotomy. Nephrotomy implies the development of more or less peri-nephritic tissue, which he will find marked in these cases, which necessarily complicates the completed operation when once it is undertaken.

Dr. McGuire's suggestion as to suprapubic cystotomy is one which has not previously occurred to me in connection with exploration of the ureters. I concur in its practicability, and shall seriously consider its adoption when occasion offers. While I recognize that it will in certain cases place the conditions under much more ready accessibility, yet in the particular case in which I made an incision through the vesico-vaginal septum, the criticism would hardly hold good.

Dr. Eastman's treatment of his case is precisely the treatment to which I alluded when I said I held under advisement the question of swagging the end of the ureter into the bladder by the plastic process, which the doctor performed with so much skill and satisfaction. I had already in my case cicatricial contraction, which was resulting in hydro-nephrosis, a contraction which I felt would not be relieved by this plastic process, and the fact that such contraction is not relieved is illustrated by Dr. Eastman's case, in which he has had occasion to practise dilatation twice.

I feel that Dr. Bedford Brown's contribution to the general subject of ureteritis is one of the most valuable that has been offered in recent years. His careful study and investigation of the symptoms, as recorded in our last volume of *Transactions*, are well worthy of perusal, and have been of value to me.

I thank the Association for the kind indulgence they have given my paper.

SPECIALISM IN MEDICINE, PARTICULARLY AS RELATED TO SURGERY AND GYNECOLOGY.

By William Warren Potter, M.D.,
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The subject of specialism and specialists has often before been presented to medical societies and other organizations, but as a rule not by specialists nor before special societies. The general practitioner, manifesting concern with regard to his hold upon his *clientèle*, has frequently resorted either to society papers or magazine articles for the purpose of attacking specialism or defending generalism, one or the other, or both. It must be admitted that in the consideration of this question, like most other questions, there are presented two sides to many of its prominent features. It is my purpose in seeking the ear of this Association, which is composed of specialists both in surgery and gynecology who are second to none in their respective departments of medicine, to discuss the subject with deference to all interests involved.

It is probable that specialties in medicine have been practised from a very early day, but you will hardly expect me to consume valuable time in searching ancient history for the purpose either of displaying a knowledge of medical classics or ministering to a taste for the curious in literature. Time is too fleeting, duties are too pressing, and life is too busy with affairs of the present to tolerate questions other than practical in meetings like this. It seems to me that the time has passed when we may with propriety discuss the need of specialism and of specialists, because it must be admitted

on all sides that they have grown to be a necessity; hence it would be difficult or impossible to return to the old way, even if we would. It cannot be expected in the very nature of things, and under the manifold conditions that are governing the present, not only with regard to medicine but also the collateral sciences, that any one man in his study, observations, or practice can cover the whole professional field in any department. If this is admitted, then a remedy must be sought whereby the people, whose servants we are, may have relief from the dilemma in which they find themselves. That remedy is manifestly offered in the division of duty, study, and practice, whereby the sick and suffering may obtain the benefits of all that is known to science as it advances from day to day.

Conceding, then, without further argument, the propriety of a division of the practice of medicine into specialties, the next question impressed upon us is: How can this best be done to promote in the highest degree the interests of the people and at the same time conserve the not less important interests of the profession itself? In championing the interests of the people, as it always ought to be the pleasure of every one to do whenever voice and pen seem called in requisition, we must not lose sight of the fact that their best interests are served when we at the same time look well to our own individual and collective interests as a profession. Of what good is it to undertake to promote the general interest, if in doing so we trample on the particular interests? Is not the general interest made up of the particular interests? If the particular interests are injured, does not the general interest suffer? Then let us accept the proposition at the outset that in laboring to promote and maintain the highest interests of the profession of medicine we at the same time bring the best guarantee to the people of an enlightened conservatism of public as well as individual health and strength.

THE TUTELAGE OF THE SPECIALIST.—Having conceded the necessity of a division of labor within the lines of the field of medicine, the next prominent feature connected with

the subject of specialism is the proper method of preparation for its practice. It has been my observation, and I presume most of you have noted the same, that here and there often spring up men who claim to be specialists who either have no preliminary preparation for the work, or else have gone out of the medical schools and walked immediately into the line of special practice without any experience as general practitioners. Either of these roads to specialism is open to condemnation. A man who offers himself to the community as a special practitioner should undergo a careful preliminary training in a medical school that is amply equipped to teach him everything known connected with anatomy, physiology, chemistry, obstetrics, surgery, medicine, hygiene, pathology, microscopy, bacteriology, and all laboratory work necessary to amply and considerately impart to him proper knowledge for the practice of medicine and surgery in any and every branch.

Having grounded himself well in the foundation principles of medicine, he may then, after his graduation, very properly enter a hospital for a service of two years ; and I pause here to remark that I do not believe this service should be performed in a special hospital, but in a general hospital, one that receives all classes of patients; for the young aspirant then especially needs the clinical experience that the wards of such a hospital afford. After his hospital service the next step should be to associate himself, if possible, with a general practitioner, either in town or country, or, if not, then he should start out for himself in the general practice of medicine. If he is compelled to do the latter the country, in my opinion, affords the best opportunity in which to obtain his first experience. He can easily spend five or eight years in this manner to great advantage, after which, he may with propriety, enter a post-graduate school for a reasonable term, and then, with further propriety, spend six months or a year abroad. A visit to Europe at this time is recommended, not so much for absolute study as for observation; but careful notes should be made of everything seen, and the record preserved for future reference. Thus, by a somewhat circuitous route, we have finally arrived at a point,

ten years after graduation and fourteen years after beginning the study of medicine, which will, other things being equal, find most men prepared as adequately as possible for the practice of a chosen specialty.

During the two years spent in hospitals, the six years in private practice, one year in post-graduate study, and another year abroad, a man will most likely have come, by the law of natural selection, to make an intelligent choice of a specialty, and in this as in most other occupations, a man should pursue that which appeals to his intelligence, aptitude, and preference. It need hardly be affirmed that a man does a thing better when he does it willingly, and because he likes to do it.

A man practises a speciality—and be it understood, I am now speaking of specialties in general and of no one in particular—better by reason of having the equipment that a large experience in general practice gives him. The constant danger in specialism, to borrow an illustration, is that a man in examining his patient will discover a pimple in the ear, and, limiting his observation to the area of his otoscope, fail to discover that there is a pimple on the nose also; in other words, he will fail to recognize the fact that a pimple in the ear merely implies a fault in the general system, and that, though it may contribute to the comfort of his patient to extinguish the pimple by a local application, it is sure to be followed by another in the ear or elsewhere, unless the remote or constitutional cause is removed. The constitutional origin of many local diseases cannot be disputed, but the specialist who is not broadly trained in general medicine will fail to recognize this fact as soon as his neighbor who has gone through such a period of preparation as I have previously indicated. What man in this presence would hesitate to choose between two specialists admittedly of equal skill in technique or pathological knowledge, but who were widely apart as to general experience? The specialist needs something more than a knowledge of technique, or pathology, or even skill in diagnosis. If he is called upon in the pursuit of his practice as a specialist to do operative surgery, he must not only possess skill and deftness

in the use of his instruments, but must have, what I consider far more important, a surgical judgment that informs him not only when and how to operate, but oftentimes when to decline to operate.

It must be admitted that the natural tendency in specialism is to limit its vision to the local malady and to overlook the general dyscrasia or the remote lesion upon which its existence depends. The human system is a wonderfully complicated machine; no one part is independent of the other; the heart suffers with the brain, and the brain with the heart; the nerves and arteries are distributed and woven into every part of the fabric, and while it is necessary for the physician to study every organ in detail, it is equally necessary that he should study it as a whole when all the parts are put together and are in motion.

This, then, is the sum of the argument, and we find it difficult to understand how a man can justify himself in beginning the practice of a specialty unless he has submitted himself, after careful preparation in the schools, to that stern tutelage which belongs to eight or ten years of general practice.

The Relation of Specialists to General Practitioners.—We have heard it said in some quarters that there has sprung up an antagonism between general practitioners of medicine and specialists. It seems unfortunate that this issue should come; but having arisen, specialists themselves must put it down. A true and honest specialist will so conduct himself toward his *clientèle* as to strengthen the hands of the general practitioner, since the latter, in nearly every instance, sees the cases first, and is responsible for their diagnosis and treatment during a considerable period of time. Finally, when matters seem to make sorry progress, the question often arises in the mind of the patient or his or her friends, whether it would not be best to consult some physician especially skilled in the line of disease now affecting the invalid. A well-trained general practitioner, taking quick advantage of the situation, will not hesitate to recommend a specialist of undoubted skill and honor. The greater the discretion he exercises at this

juncture the stronger will be his hold upon the family. If he sends his patients to a consultant who brings success in the care of their maladies it reflects upon the skill and judgment of the family doctor in a high degree, and the converse is equally true. That physician who is quickest to recognize the necessity for the opinion and aid of a specialist, not waiting for the patient to move in the premises, but promptly calls to his support the strongest and most honest man within his reach, and invokes his special skill in the conduct of a needful case, will thus avoid blame for delay, or, mayhap, disaster, and moreover, take the strongest hold on the affections of his *clientèle.* It is a common habit among specialists to make remark of the family physician in this wise: "My dear madam, you have fallen into fortunate hands; your physician is a man of skill and experience, and while no man, however wise and erudite, is competent to cover, either in his diagnosis or treatment, the whole field of medicine, your physician will always, in doubtful cases, select as counsel men who will bring substantial aid in your distress and advancement of your interests." Such suggestions are always sure to strengthen confidence in the family doctor, and reflect credit upon the specialist.

When all interests concerned in this phase of the question recognize the fact that specialists need help from the general practitioner quite as often as the general practitioner needs help from them it will be better for physicians, general and special, as well as their patients. A duty that is imperative, one that the specialist cannot escape or shun, is to resign all general practice upon entering a special field. Unless this is cheerfully and wholly done the general practitioner is not treated fairly, and he will not be slow to recognize the fact. There is no real conflict between the general practitioner and the specialist, and it must not be allowed to appear that there is by any coloring that is thrown upon the screen which perverts the facts. This is a side of the question that would permit of much elaboration, but on this occasion it must suffice for me to make only this passing allusion to it.

Reciprocal Relations Between Specialists.—Not second in importance to, but quite abreast of, the preceding subdivision is the consideration of the relations of specialists to each other, and considerable observation and experience have served to convince me that there is much opportunity for thought under this head, as well as for improvement in these relations. In the presentation of this part of the subject I shall especially consider it with reference to the departments of medicine which fall within the particular province of this association to discuss, namely surgery and gynecology.

To-day all will acknowledge the fact that the tendency of the successful practice of gynecology is toward the surgical side of the subject. It is as indispensable for the gynecologist to be a practical surgeon as it is for the physician to be a gentleman. Not that every gynecological patient must needs have surgical aid, but as we come to understand the pathology of the pelvis and abdomen better, we appreciate the fact that the requirements of minor gynecology are becoming fewer and less. It is not many years since the possession of a Fergusson's speculum, a caustic holder, a uterine dressing forceps, and a Simpson's sound were considered by the majority of physicians an ample outfit for the practice of gynecology. If, indeed, there were a favored few—favored either through location of residence or perfection of training—that added a number of other instruments to their armamentaria, it still remains true that in the hands of the majority who made pretence to practise the art the instruments named were considered all-sufficient for the diagnosis, treatment, and cure (?) of the sexual ills of women. It was common then for the teacher of gynecology to spend much time in the description of the methods of use and the multifarious applications to be made with the uterine sound. The opinion of the greatest number of gynecologists now is, that there is only very rarely a necessity for the employment of this dangerous instrument, and that, on the whole, it is one of the few inventions that women would be the better for if it had not been discovered. An im-

portant function of the teacher of gynecology to-day is to lay special condemnation on the frequent employment of this instrument and to warn against its dangers.

Modern gynecology, too, must teach the modern pathology of diseases of the abdomen and pelvis, and as we come to more intimately understand this pathology, together with an intelligent etiology of pelvic disease, we more and more frequently invoke the knife in promoting or establishing the cure of these maladies. Hence, there is, I say, an intimate relationship between surgery and gynecology. If I also pause to affirm that there is, likewise, an intimate relation between surgeons and gynecologists, it may not be out of place to remind you of the fact—one that all of you well know—namely, that this Association is the only existing special organization where these two departments of medicine are solely and distinctively united in society work.

The Ethics of Specialism.—From this very fact it will become the province of this Association to be the leader in cementing the relations between surgery and gynecology and the expounder of the ethics that should govern the relations of these important subdivisions of the medical field. In this use of the term ethics I do not wish to be understood that there must be written a new decalogue of ethical dogmas, but instead I desire to bespeak a revival of the principles of Confucius who, 500 years before our Saviour, formulated what has ultimately come to be regarded as the Golden Rule. If Confucius is correctly translated he said, "Do not do unto others what you would not have them do to you." This is one of the inner truths, and may be regarded as the embodiment of all justice. Nevertheless, Chinese edicts, Roman pandects, Napoleonic codes, English common law, and American jurisprudence, each and all of them have failed to give it force in practice. I might have added, for the sake of accuracy or to escape the imputation of carelessness, that that eminently wise, conservative, and forensic tribunal—the Supreme Court of the American Medical Profession—the Judicial Council of the American Medical Association, has even proven itself equally

as incompetent to give this stalwart truth an interpretation that is acceptable to all, and as has signally failed to enforce its precepts.

In 1840 Samuel Jackson, that immortal physician, in addressing a class at the University of Pennsylvania said, "Every man of good sense, possessed of honorable sentiments and a moral feeling of right and wrong, by the instinct of honesty will know how to conduct himself without a code to regulate his deeds." This utterance, by a man whose daily life was an epitome of ethical propriety, expresses all that is needful to say on the subject, and is adapted alike to guide the specialist and general practitioner.

When a gynecologist spends four years in college attendance, ten years in private practice, post-graduate study, and travel abroad, preparing himself under the law of natural selection for the specialty of his preference; when he abandons all his previously conceived notions of practice; abandons bone surgery, and the treatment of diseases of the nose, throat, eye, ear and other organs of the special senses; ceases to treat diseases properly belonging to the neurologist; gives up fractures and dislocations, amputations and the like; when, I say, he abandons all these branches of the practice and sends patients suffering with these several maladies to those special surgeons who are skilled in the several departments named, in order that he may devote himself exclusively to the practice of abdominal and pelvic surgery and the treatment of gynecic diseases, is it not reasonable for him to expect a reciprocity of action on the part of his friend and brother, the general surgeon?

There are, happily, many individual instances where this reciprocity is recognized to its fullest extent; but I fear that many surgeons are either careless of or indifferent to the amplest realization of the importance of maintaining a high standard in these reciprocal relations. When the gynecologist turns over to his brother, skilled in the treatment of diseases of the joints, the injuries of this nature that apply to him, does he not fairly expect in return that the removal of abdominal

and pelvic tumors and the like will be declined by the specialist in surgery of the joints? When the gynecologist refers all brain, neck, throat, and chest surgery to the surgeons especially skilled in these several departmeuts, he certainly does not expect that these same surgeons will open the abdomen or invade the pelvic basin in women on every and any pretext that can be found for so doing!

It cannot be expected, however, that the general surgeon will feel altogether like giving up a field so attractive as the pelvis and abdomen without some reciprocal compensation. Fortunately, there is much remaining of interest for him to cultivate. If he will considerately yield to the gynecologist everything pertaining to the sexual diseases of women which it must be confessed falls within his domain, he can still cultivate the fruitful field that all abdominal wounds afford, and perfect himself in the technique of suturing the intestines, intestinal anastomoses, resection, and the like, together with many other surgical diseases and accidents involving the necessity of abdominal section. It seems to me that it is very important to draw the line very accurately in this matter. The removal of all abdominal tumors, and all surgery connected with the uterine adnexa, has been the subject of special study by the gynecological surgeon; he was the first to point out its possibility, to develop its proper technique, and to apply rigidly the rules of simplicity, cleanliness, and drainage that have brought to success its practice, and reduced its mortality to a small fraction in the hands of competent men; for, whereas, only a decade and a half ago it was constantly our habit to estimate a record as eminently gratifying with a mortality of even 25, 33, or 40 per cent., now we find it has been reduced to an exiguity of 3 per cent. for all operations, favorable and unfavorable. To say that these results have been mainly due to the indefatigable energy of Keith, Tait, Bantock, Price, McMurtry, and others well known to you, is only to render unto Cæsar the things that are Cæsar's. Shall not these men and their coadjutors and disciples, in all fairness, be entitled to the opportunity of pursuing this field exclusively?

When we recall the almost hermit-like exclusion that a man must submit himself to in preparing for this work and in perfecting his technique, together with the dangers that he must encounter in its prosecution, besides the mental and physical strain that he must undergo; then add to all this the limited income that he will receive during the earlier years of this stern tutelage, we can but regard it as almost a matter of justice that his claim to the territory under discussion shall be admitted and all its boundaries respected. Moreover, it has been demonstrated by results that the general surgeon cannot successfully cope with the gynecic surgeon in pelvic surgery.

I hope that I have not been misunderstood in this matter, for no man has a higher respect for the general surgeon than have I. During a number of years in my earlier life it was my fortune to occupy a place within the ranks of that able and accomplished circle. During the last few years, however, my surgical preference has lain within the lines of gynecology. It is perfectly fair for me to state, then, that I do not allow prejudice to enter into this critique. I am only candidly stating my own views on the subject, and I hope you will accept them for what little worth there may be in them. My object has been to contribute something toward the cure of an evil and to ask this Association to take the subject into consideration and to deal with it as it thinks best. I may have already spoken too long; perhaps I have said more than I ought to have said, but yet there are many sides of the question, and many of its bearings that have been left untouched. I stand here only to make a plea for justice on all sides and not to antagonize any interests, individual or general.

Responsibilities of the Specialist.—The means of education at command, both for specialists and generalists, are almost limitless; the schools are extending their terms of study and otherwise improving their curricula; most of the States have established separate examining and licensing boards that in effect determine the equality of the teaching in the schools and stimulate them to maintain a uniform standard.

The massiveness and excellence of medical literature were never so high; the clinical opportunities for perfection were never so great; in short, never before could there be obtained such a high quality of preparation in the schools. The general practitioner now receives his certificate of study, and graduates with a perfection of knowledge that is even in excess of that which most specialists could boast of twenty years ago. With such immense gain in the equipment of the general practitioner, he is placed at once in competition with, and soon will drive out the inferior or indifferent men who pose as specialists; as, indeed, such ought to be driven out. The specialist of to-day, therefore, is expected to be something more than was demanded a quarter of a century ago.

The tendency of the schools is to encourage specialism; in my opinion this tendency ought to be restrained. The schools should be thinking less of fitting students for the practice of specialties, and pay more attention to their equipment as general practitioners. Specialists will evolve fast enough from these general practitioners after they have become sufficiently experienced in general medicine, and the only road to the practice of a specialty should be through this channel.

Conclusions.—A few words to specialists in general, that is, to you and myself, and I have done. No man has a higher appreciation of the work done by specialists than have I. I am not here to condemn, but rather to encourage them in their work. Some of their greatest triumphs have been achieved in the State of Kentucky, and within an area almost compassed by the sound of my voice. When Benjamin W. Dudley mastered the difficulties of stone in the bladder he performed a service that will hand his name down the ages to the very crack of doom; when Ephraim McDowell solved the problem of ovarian cysts by curing Mrs. Crawford, in 1809, in spite of personal threat and imminent danger, he accomplished something that will cause his name to "spin forever down the ringing grooves of change."

When the specialist in mental diseases restores an insane person to reason he accomplishes something that lays human-

ity under obligations. The etiology of many nervous disorders in women have been long understood to lie in the disturbed functions of the sexual organs, and the associated mental disease has been recognized by the gynecological specialist as only a symptom. These cases usually come first within the observation and management of general practitioners whose skill is taxed in diagnosis and treatment. They are generally correct in both. It must be claimed for gynecology, however, and some of the men whom I see here present are pioneers in the work, that it has evolved the enduring truth that many of these mental disorders are complicated with sexual disease, and that if the sexual disease is cured, reason is restored. This subject is still under judgment, and promises a rich future for the worker in this field. It has already achieved sufficient success to give promise of yet greater results.

I repeat, that the responsibilities of the specialist were never so great as to-day, hence his training should be of the broadest and most liberal kind, and he also should be moulded after a broad and liberal model. He should not be of a jealous nature, and he must bear in mind that there is no antagonism between himself and the general practitioner. If the latter is becoming somewhat irritated over the fact that tramps have taken possession of some of his territory, his consolation must be that there is yet remaining enough for him to cultivate, and no one will be more happy to assist in driving off these tramps than the accomplished specialist.

The argument, then, that I desire to impress is:

1. There is essential need for specialists. Divisions of labor in every field are demanded, and nowhere more than in medicine.

2. Specialists being a necessity, they must amply equip themselves by years of study, and devote themselves to a still greater number of years of general practice before they are justified in offering themselves as specialists.

3. They must conduct themselves in such a way as to merit the respect of the general practitioner, and to invite his co-operation in their work.

4. The unwritten ethics of specialism demand that there shall be a reciprocal relationship maintained, not only among specialists themselves, but also between specialists and general practitioners.

5. The opportunities for perfection in special lines of medical study are so great, and medical literature in both journalistic and text-book form is so rich, that a weighty responsibility is entailed upon the specialist, and his duties must be discharged with fidelity and honor.

6. The schools ought to discourage any and all students who give promise of entering upon the practice of a specialty as soon as the college doors are passed and before the swaddling clothes of the professional tyro are slipped.

7. A kinship between surgery and gynecology—between the general surgeon and the gynecological surgeon—ought to be strongly cultivated and stoutly maintained. Neither should trench upon the territory naturally belonging to the other, and to conserve these reciprocal relations will assuredly be the pride of this Association.

THE GENERAL PRACTITIONER AS A GYNECOLOGIST.

By R. M. Cunningham,
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To know one's duty and to possess the capacity to execute it is an axiom applying to every relation of life, but more particularly to the medical man, because he deals with human health and life. That the science of medicine, using the term in its comprehensive sense, thoroughly understood and properly applied in practice, always does good and often saves life, cannot be questioned. That an imperfect knowledge of this science, or its improper application in practice, may result disastrously, also cannot be questioned. Therefore, to comprehend thoroughly the principles of medical science, and to skilfully apply them, is the moral, conscientious, and should be the legal requirement of every practitioner of medicine and surgery. The subject, as a whole, however, is so vast, both in its scientific and practical relations with the diseased, embracing every organ, tissue, and system of the body, with their multifarious forms of disease, differing in etiology, pathology, symptomatology and treatment, that the mind of man cannot comprehend the whole problem and apply in practice the best and surest treatment, consequently, specialisms necessarily follow. Hence, if humanity enjoys the full benefits at the hands of the profession which the capabilities of the science and art confer, the appellation, "General Practitioner" in its most comprehensive sense exists only in theory, and not in fact. Yet there is such a thing as a

general practitioner, and who, in the restricted sense of doing the best he can, is found everywhere. Specialists who deserve the name are not found everywhere, and to the majority of the people in many places are not accessible; therefore, upon the general practitioner such people must alone rely for the benefits of the medical profession, or suffer and die without them. Such an environment embraces a large percentage, if not a majority, of the people of this country. The general practitioner thereby becomes the most important and useful member of the profession. Indeed, he is one of the rank and file of the vast army of medical men who are officered and directed by the specialists in the various departments of the profession.

In our large cities and towns the general practitioner's duties become very much restricted. Within a few blocks he may send his patients to the oculist, aurist, neurologist, gynecologist, etc.; his surgical cases to the general surgeon; charity cases to the public hospital, but in the country and smaller towns there is no such favorable environment. The most of his clientèle are financially unable to visit the larger cities to consult a specialist; there is no public hospital for his cases of charity. Therefore, he must do all that can be done under the circumstances.

Of all the the specialisms, gynecology stands at the head. No department of the profession has accomplished more. No specialist stands higher nor is more revered than the gynecic specialist. No country has done more in pioneer gynecology than America. No country, in any department of the profession, can show brighter and more illustrious names than McDowell, Sims, Batty, Emmet, and others. No country can present, at the present, a more illustrious faculty of gynecologists than America, many of whom are members of this Association. Gynecology is, therefore, of great professional interest to the American general practitioner, both in theory and practice. To know his duty, what can be done, what ought to be done, and what he can do with safety to his patient, and with the promise of success in gynecological cases,

is one of the most important problems that can possibly confront him.

This paper is written for and in behalf of the rank and file of the profession, and is read here in the hearing of some of the most distinguished gynecologists in this country, with the hope that it may elicit such discussion, pro or con, as will throw some light from an authoritative source upon the duties and limitations of the "General Practitioner as a Gynecologist," applying to that large class of the profession who practice in the country or in the smaller towns.

The special environment of such a practitioner calls for some remarks. In the first place, most of his acts are subjected to the severest of all criticisms, to wit: That of incompetent critics. Anything that he does that is not in keeping with the popular idea of the matter in hand will be looked upon with suspicion and distrust, and should an accident happen, woe be unto him! Let it be said with regret that such results, however inevitable, are not always defended by the other physicians in the neighborhood. So that a greater amount of courage and moral heroism is at once required of him. He has no hospital, skilled assistants, nor experienced nurses. His work must be done in the homes of his patients, with such assistance as his local confrères are willing to give him, and with the family or some good Samaritan who is not always clear of gossip in the neighborhood, for nurses. Such an environment necessarily limits the work of the general practitioner, not merely from policy, but principle. Notwithstanding, in my judgment, he has a legitimate and necessary field of work, to which I briefly call attention. My remarks, of course, apply only to the instrumental treatment of this class of cases. So important a subject cannot be considered in full, nor in detail, but in a general way only.

1. The general practitioner should not undertake work that can be better and more safely done by the specialist, provided one is attainable.

2. He should be willing to do and attempt the most radical and dangerous operations when necessary to save life, pro-

vided a specialist or one better prepared to do the work cannot be obtained.

3. In cases not necessarily dangerous, or in which life does not become more or less a burden, but in which a cure can be effected only by a radical procedure, but which may be materially benefited or symptomatically relieved by milder methods, he should adopt the latter and not the former.

4. In many cases the field is clearly his own, belongs to him, and he should be prepared and competent to treat them with safety and success.

The most important, certain, and effectual work of the gynecologist is his abdominal and pelvic work. In no department of the profession, perhaps, do the advantages of skilled work, nursing, and suitable environment, play as important a role in obtaining results as in this class of work. Therefore, experience and preparation are essential requirements in electing to do laparotomies. Hence the general practitioner, unless he be a laparotomist in embryo, with aspirations to become a specialist, and must have experience, and, therefore, victims, should never elect to do this class of work. Yet, he has a field for this class of work. No courageous, honest man would allow a woman to bleed to death from a ruptured tubal pregnancy, a fatal peritonitis from a ruptured abscess, or a general septic peritonitis in puerperal cases, etc., without giving his patient a chance for her life by promptly opening the abdomen and meeting the clear indications there found. Nor would he allow a woman to die with an ovarian cystoma, or bleed to death from fibroids, when a specialist cannot be obtained, without removing the former, and at least performing Batty's operation in the latter, provided he would not dare do hysterectomy. In a general way, then, it may be said that laparotomy work should not be done by the general practitioner, except in cases of emergency and when a specialist cannot be obtained.

Cancer of the uterus is pre-eminently a case for the specialist, because, complete removal of the organ is the most promising treatment for this class of cases.

Secondary or plastic operations upon the perineum, vagina, and cervix, while not dangerous, require more than ordinary skill to obtain good results, and, therefore, belong to the specialist. No one can observe a number of operators do this kind of work without being convinced that even among specialists there is a wonderful difference in the skill displayed, and also of the results that follow.

Electricity does not promise much under any circumstances in gynecology, and its use by the general practitioner is of doubtful utility and safety.

In the third class of cases, not necessarily dangerous to life, and in which life is not a burden, but for the permanent cure of which a radical operation is necessary, and for the improvement or symptomatically relieving of which milder measures may answer, the general practitioner has an important field of work.

First and most important of this class are those cases of pelvic inflammation in which, as a rule, there is more or less displacement of the uterus, which is fixed by adhesions, together with more or less tubal and ovarian involvement, all being more or less fixed in unnatural relations by the coincidental pelvic peritonitis.

Now in such cases, if there be collections of pus in the tube, ovary, or encysted in the peritoneum, palliating measures are not only of little service, but some of them may be dangerous. Therefore, a laparotomy and removal of the diseased appendages and pus collections is the proper treatment. But in such cases when the pus is drained, *e. g.*, a pus tube into the uterus or pelvic abscess through the vagina, laparotomy may not be necessary.

Now, I believe that these are the cases, particularly if of long standing and recurrent, in which the only cure is a laparotomy, removal of the diseased appendages, breaking up of the adhesions, ventral fixation, or some other operation to keep the uterus in place. That there are cases, however, in which complete resolution occurs, with restoration of function, I have no doubt. But they are very rare. What can the gen-

eral practitioner do? Until recently, in my judgment, he could do very little. Hot water douches, painting the cervix and vault of vagina with iodine tincture, vaginal tampons soaked in glycerin, boro-glyceride, etc., salines during the acute attacks or exacerbations, and general treatment to improve the health, formerly constituted his chief reliance. That many cases were thus symptomatically relieved; that the pelvic exudates are sometimes diminished, and even partial mobility of the uterus restored, in fact, an improvement in both symptoms and pathological condition, I can myself testify; but the treatment to be of service must be long continued, is uncertain, and not very promising.

Professor Polk, of New York, in my judgment, has greatly simplified the treatment of these cases by the use of the sharp curette, gauze packing, and drainage. By it more can be accomplished at one operation than can be done, if done at all, in months by the older methods. My recent experience has convinced me of the great promise, utility and comparative safety of this operation. I am aware that this treatment is an innovation, that the use of the probe, curette or any disturbance of the endometrium when there are peri-uterine inflammations, has been almost unanimously condemned by authorities. Notwithstanding, sufficient experience is fast accumulating to prove, not only the utility, but the safety of this procedure; this treatment, followed up by the usual methods, will, in my judgment, absolutely cure some cases and relieve symptomatically the majority. This operation can be safely performed by the general practitioner who has ordinary skill in the use of instruments, and who can apply in practice the principles of aseptic surgery.

Among the fourth class, or cases which properly belong to the general practitioner, may be mentioned the various forms of uterine inflammation, cervical, corporeal, and general endometritis and metritis (subinvolution and areola hyperplasia). These diseases, if neglected, may result in tubal or ovarian diseases, and consequently in pelvic peritonitis, with displacement and fixation of the pelvic organs. The causes of these diseases

in the majority of cases are to be traced to labor or miscarriage. Sepsis, as a rule, is the essential cause in the former, either causing an endometritis resulting in subinvolution, or possibly in the perineum structures, resulting in subinvolution and endometritis. In miscarriage, in my judgment, the main cause is the retention of more or less of the secundines, resulting in septic infection, inflammation, or in subinvolution than in endometritis. In such cases the probability is that there are either the remains of physiological structures that have become pathological, or structures that are the result of pathological processes, in both cases structures foreign to the uterine cavity. Here the indications are clear.

1. To rid the uterine cavity of all foreign structures.

2. To cause contraction in the subinvoluted uterus, thereby restoring normal circulation and causing an absorption of hyperplastic elements.

3. To establish sufficient and adequate drainage, while the physiological process restores normal structure and function. These indications are promptly and effectually met by the curette and gauze packing. That the curette is an instrument of great practical importance is attested by the fact that almost all the authors recommend it as a last resort in the treatment of endometritis and as an essential instrument in the treatment of certain forms of that disease. That the instrument can accomplish much in suitable cases, the testimony of all proves. The question therefore arises, Is the curette, particularly the sharp one, a safe instrument in the hands of the general practitioner? As formerly used by both specialist and general practitioner, it was undoubtedly often followed by bad results. That penetration of the walls of the uterus occasionally occurred in the hands of the most experienced and skilled; that peri-uterine inflammation sometimes followed, are admitted. It may, therefore, be regarded as an instrument of danger if improperly used. If, however, it is made to play only its part in a thorough surgical operation, embodying all the principles of modern surgery, then it becomes no more dangerous than other surgical instruments in the hands of an ordinarily skilled

operator. I, therefore, conclude contrary to the teaching of authors, particularly Emmet and Thomas, whose writings have done more to formulate opinion in gynecology than any other authors in this country, that the use of the dull and sharp curette is safe in the hands of the general practitioner, and constitutes the most important and essential instrument in his gynecological armamentarium, provided he has ordinary skill in the use of instruments and uses it as advised by Dr. Polk, that is, in a thoroughly surgical manner.

The many indications for the sharp curette, the prompt and effectual results that usually follow its use; the almost universal opinion of gynecologists that it is an instrument for the specialists only, and therefore, should be seldom used by the general practitioner, are the principal reasons I offer as an excuse for this paper, with the hope that such criticism will follow as will untie the hands of the general practitioner so far as the use of this instrument is concerned, that he may feel justified to use it in the treatment of many cases that he has failed to cure by the usual methods, thereby curing many, thus preventing tubal and ovarian disease, and symptomatically relieving many whose only hope of cure is by laparotomy, regarding it always as a part of a surgical procedure consisting of dilatation, douching, curetting, and gauze-packing, and drainage, with strict aseptic precautions, and as a rule, under an anæsthetic.

In concluding this part of the subject I give it as my opinion that the introduction of a dirty probe, curette or other instrument through a dirty vagina and cervical canal into a possible septic uterine cavity is not only "gynecological tinkering," but practically malpractice. Hence, all such procedures are surgical and should be guarded accordingly.

In concluding, there is one other thing to which I desire to call attention, although it may be regarded as obstetrical, and that is the immediate repair of lacerations of the tissues of the genital outlet occurring during labor. According to my observation, this is as a rule neglected. So far as the general practitioner is concerned, there are two main reasons

why this is neglected. First, the majority of the lacerations either get well without operation, or, according to his experience, give no trouble afterward. Second, it is a confession upon his part that there has been a tear, which is seldom discovered by the patient or her nurse, and which is regarded by her and her friends as an evidence of ignorance or neglect on his part; therefore, he generally does not look for them, or if he does and finds them, he ignores them. These remarks do not, of course, apply in cases where the sphincter is torn.

Now, I believe that the majority of women have more or less laceration, but that the majority of these suffer no consequences. Hence, I would not advise that it is always necessary to repair them, but I do insist that it is the duty of the general practitioner to examine every case, and if a lesion is found involving the deeper structure of the outlet, the fascia, muscles, etc., or in case of a comparatively extensive tear, he should advise his patient of the fact and insist on repairing it. At this time the operation is a simple surgical procedure of bringing together divided structures by the various forms of suture, buried, interrupted or continuous, before retraction and cicatrization alter their normal relations, thus providing against the necessity of a plastic operation, which requires great skill, and before secondary lesions follow, attributable to the laceration.

So far as the implied confession of ignorance or neglect is concerned, which Professor Lusk says merits no consideration, much depends upon the popular idea concerning the preventability of these lesions. If the prevalent idea in a community is that one of the duties, if not the main duty, of the physician is to prevent these lesions, and that he can do so if he is educated in his profession, and if the physician is not established in reputation to render harmless adverse criticism (and who is?), and if there be a competing brother in the community who has had hundreds of cases without a tear (and where is he not found?), then I say it is one of importance and deserves consideration, and if womankind is properly treated, cannot be ignored. I believe it to be the duty of the

profession to teach that the popular idea of supporting the perineum is a fallacy, and that the professional efforts in preventing tears are very largely futile, consequently that lacerations will occur in anybody's practice, and that it is no necessary reflection upon the knowledge or skill of the attendant when they do occur. Where the necessity of primary operation for these lesions is properly appreciated by the general practitioner and realized by the laity the plastic work of the specialist will greatly diminish.

In conclusion, I submit that the general practitioner can largely diminish the work of the specialist, particularly his pelvic work, by the application of well-known surgical principles in the treatment of many of the local diseases following labor and abortion.

If I have succeeded in this paper in calling sufficient attention to this important subject, that it (the subject) may be fully and freely discussed by the eminent authorities present, and if the consensus of opinion here expressed tends to limit the work of the general practitioner in work for which he is unfitted and unprepared, and which he should never do except in emergency, that is, when the specialist cannot be obtained, and on the other hand, to enlarge his field of work in certain directions, particularly in the use of the sharp curette as indicated in this paper, I shall have accomplished my purpose, and, in my judgment, benefited the patients of this class of cases and conserved the interest of both practitioner and gynecic specialist.

A PRELIMINARY REPORT ON THE MORPHOLOGY OF OVARIAN AND MYOMATOUS TUMORS.

With Numerous Lantern-slide Illustrations.

By Howard A. Kelly,
Baltimore.

I will offer no apology for presenting for the consideration of the Southern Surgical and Gynecological Association any facts whatever relative to the life-history of ovarian and myomatous tumors, affections which are so constantly before the members from a severely practical standpoint.

I will use the word morphology in a broad senes, to include changes in contour caused by these tumors, as well as peculiarities in the form and disposition of the tumors themselves. I propose, therefore, briefly to consider certain characteristic peculiarities produced (*a*) in the form of the abdomen itself; (*b*) peculiarities in the disposition, or packing away of these tumors within the abdominal cavity, and (*c*) peculiarities of form assumed by these tumors in so far as they are affected by the constraining, constricting influences of their environment, in other words, the extent to which they are capable of being moulded by the pressure of neighboring structures.

CHARACTERISTIC TRAITS EVIDENT UPON INSPECTION OF THE ABDOMEN.

The general tendency of tumors growing out of the pelvis and extending up into the lower part of the abdomen is to produce marked distention of the lower abdominal zone, at once

evident to the eye, and readily recorded by measuring the distance from the umbilicus to the anterior superior spines and symphysis, which is increased beyond the normal, while the distance from the umbilicus to the sternum and ribs remains but slightly, if at all, changed. This generic difference I now demonstrate by these lantern-slides comparing a group of tumors arising from and distending the upper part of the abdomen, with a pregnancy eight months advanced, taken as a type of tumors arising from below.

The form of abdomen characteristic of large ovarian cysts is a globular or ovoid distention of a part or the whole of the abdominal wall, pushing out the infra-umbilical portion much more than the supra-umbilical, at least so long as the tumor occupies the lower half or two-thirds of the abdomen. This enlargement is uniform in parovarian cysts and polycystic tumors exhibiting but few bosses, due to the fact that the latter are composed of one or two large cysts associated with a mass of smaller ones, and the large cyst is best accommodated in the median line in the distended concave anterior abdominal wall, while the smaller ones at the side or back consequently do not show. This is one cause of a torsion of the pedicle.

The alate or wing-shaped chest is due to a pushing out of the lower ribs, and is a characteristic of any tumor big enough to distend the entire abdominal cavity.

Prominent exceptions to the general rule just enunciated, that pelvic tumors distend most markedly the inferior abdominal zone, are the notable stretching of the upper abdomen in very fat women with large ovarian tumors, and the like distention in rhachitic dwarfs in advanced pregnancy with a *venter propendens.*

Nodular myomata, on the other hand, stand out in qutie marked contrast to the smooth outlines of cystic tumors in giving the lower abdomen a lumpy, bossed appearance, thus exhibiting through muscles and skin a softened exaggeration of their irregular outlines. This peculiarity still re-

mains prominent after these tumors have undergone fibro-cystic degeneration.

The contrast of the globular outline of the ovarian tumor with the more or less bossed appearance of a large fibro-cystic tumor is well shown here in one of our photographs.

The flattened ovoid of an ascites, which is no tumor, shows well the striking difference between an enlargement contained in its own sac wall and one free and without definite form in the abdominal cavity.

It is well to bear in mind, however, that occasionally a simple ascitic accumulation will distend the abdomen prominently in the median line, presenting a deceptive appearance, a close mimicry of the encysted tumors.

THE DISPOSITION OF TUMORS IN THE PELVIS AND ABDOMEN FOLLOWS THE LAW OF ACCOMMODATION.

The problem presented is that of a body with a smooth surface and of varying degree of mobility contained within an irregular cavity with a smooth lining and subjected to frequent movements of succussion. The law is, that the body in question finally assumes that position best suited to its form, subject to the single restriction of its attachments or adhesions.

Further, if the form of the containing body be constantly changing, the contained body may be compelled to undergo movements of rotation.

Small ovarian tumors up to the size of a goose egg commonly lie on the same side from which they originate.

Upon continuing to grow, such a cyst at first extends over to the opposite side of the pelvis, and then proceeds to enlarge upward into the abdominal cavity. As soon as the cyst leaves the side on which it originated it commences to put traction upon its pedicle, thus pulling the uterus toward that side. With this shortening of one fornix the opposite fornix is made wider, and the tumor is thus in this case more easily palpated in the vagina on the *side opposite* to its attachment,

and the natural but erroneous inference in attempting to locate its starting point is that it is a right or a left-sided tumor according as it is felt most easily on the right or left side.

The facility with which the simpler monocystic and some jelly-like tumors become moulded and accommodate their form to their surroundings may be well seen by noting the effect of gravity outside the body, where they at once collapse into a flattened ovoid mass.

In the body such cysts often fill the pelvis and bud out into the abdomen until they assume the shape of a gourd. Within the abdomen they are ovoid or flattened ovoid, according to the amount of distention of the sac.

Cystic tumors filling the pelvis and part of the abdomen are rarely found to originate in some upper abdominal tumors. I have here for demonstration a photograph of an enormous kidney, containing over a gallon (4 litres) of pus, extending from the pelvic floor up through the abdomen and pushing up the left ribs.

Polycystic ovarian tumors, left to grow until they extend above the umbilicus, if not detained by adhesions, are most apt to lie markedly displaced to the right side. The explanation of this peculiarity is the same as that I have suggested for the right obliquity and torsion of the uterus in advanced pregnancy. The tumor is displaced to the right by the stomach, the repeated soft pushing impacts of the constantly alternately, enlarging, and contracting stomach being far more potent than the tendency of gravity to keep it forward in the median line, or the tendency of the movements of the patient to dispose of it in some other position. For the same reason, in big ovarian tumors an enormous sac will often be found directly under the concavity of the liver, snugly fitting convexity into concavity. This dextro-position of the tumor will not be apparent on the surface of the abdomen when a distended stomach compensates the inequality and gives the surface a uniformly rounded appearance.

The disposition of the intestines in the case of tumors rising from the pelvis is peculiar and worthy of note. They

are at first crowded up away from the front and toward the back, and then out into the flanks. The omentum and transverse colon lie in front of or across the upper part of the tumor, which almost always thus remains infra-omental.

The disposition of myomatous tumors is interesting. Not infrequently one or more remain fixed in the pelvis, where they grow until they choke vital organs and threaten the life of the patient. Peculiar myomata are often met in women who have borne children, appearing as a more or less uniform enlargement of the whole uterine body, assuming the appearance and characteristic position of the pregnant uterus, bellying out the abdominal wall in the median line in front.

FORM-CHANGES IN THE TUMORS THEMSELVES.

Multiple fundal myomata often lie like saddle-bags, hanging one on either side in the iliac fossæ and the groins.

Although myomata are among the densest tumors of the body, they often show the impress of the structure with which they have been in constant contact during their growth in a remarkable way. Thus I have seen a distinct sulcus about sixteen cm. long, caused by the pressure of the linea alba running up the length of a large tumor. Upon extirpating the tumor I observed the sharp, unyielding fibrous linea exactly fitting this sulcus, and noted further that the sulcus did not simply arise from the junction of two myomatous nodules.

Again, I have seen a broad myoma with its back broken, as it were, bent over the promontory of the sacrum and the lower lumbar vertebræ, showing upon removal on its posterior surface an exact mould of the broad vertebral ridge.

To show how fruitful and interesting such observations may become, the following points of interest were recently culled in one day when I performed four cœliotomies for myomata. In the first case the nodule was small, lying in the posterior surface of the uterus near the fundus, which it had toppled over, causing a retroflexion ; in the second case, the myoma-

tous uterus was of the size and form, with the exception of a little greater breadth, and almost of the consistency of an advanced pregnancy. Another case was a large fibro-cystic tumor filling the pelvis and rising up into the abdomen like molten wax.

The remaining case, a multinodular myomatous mass, proved especially interesting from the standpoint of this investigation. The tumor had formed a dense attachment by one of its nodules to the rectum low down in the pelvis. *The rectum*, however, *was displaced and dragged over into contact with the right pelvic wall.* This anomaly could only be explained by assuming that the myomatous nodule had originally occupied a position in the median line, where it adhered to the rectum, and later, in some way, it had become displaced to the right, dragging the adherent rectum along with it. This interpretation was borne out by the further observation that the uterus itself was rotated one-fourth way round from right to left, so that the right tube and ovary lay under the linea alba. This torsion of the uterus was explained by the fact that the linea alba lay in a groove about two cm. deep between two large fibrous nodules, the only masses found on the fundus, one in front, a hard white nodule about five cm. in diameter, and one posterior, redder and more succulent, about eight cm. in diameter. The full explanation then is briefly this: originally there were two prominent nodules, one anterior at the fundal extremity, and one posteriorly at the cervical extremity. The latter became adherent to the bowel. Later a large nodule developed posteriorly at the fundal end, and, growing rapidly, lifted the uterus up out of the pelvis and pushed the mass anterior to the fundus forward until it met the fibrous linea alba, which, striking its smooth surface, deflected it to the left side, thus rotating the uterus and dragging the cervical nodule with the rectum over to the right side.

In giving this explanation I was confirmed by her physician, Dr. Hildebrandt, of Maryland, who has kept her under close observation for several years, and who says that the big

mass has been noted both by himself and the patient as of recent growth.

One of the most remarkable and yet not uncommon instances of this adaptation to environment is shown by a case still in the Johns Hopkins Hospital. A dense myomatous mass choked the whole pelvis, and after opening the abdomen was only extracted with patient persistence and the use of considerable force, and then only after partly breaking its pedicle near the fundus, although there were no adhesions. Its attachment with that of several other nodules was near the fundus, but the largest tumor had been caught in its growth beneath the promontory of the sacrum, where it continued to develop until all the pelvic viscera were compressed and the patient reduced to a pitiable condition. You will observe in the photograph the irregular anterior surface adapted to the cervix and lower part of the body of the uterus. The posterior surface, however, is as exact a model of the sacral concavity as could be obtained by pouring plaster into the pelvis and filling it from floor to brim. This is beautifully shown both in profile and full view taken from behind.

This brief study is only intended to be suggestive. One of my objects is to stimulate closer observation of details in these important cases which we are nowadays handling in such wholesale fashion.

THE PRESENT STATUS OF DRAINAGE IN SURGERY.

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The title of this paper, while seeming to embrace the entire subject of surgical drainage, is intended to refer to the question of primary wound drainage. Our present knowledge of the principles upon which success is founded in the treatment of septic wounds will not permit even the dream that artificial drainage can ever be eliminated from our resources as a secondary measure in wound therapeutics; hence, secondary or pus drainage will not be discussed here.

Before the researches and work of Chassaignac medical history gives us little accurate data as to the progress of drainage through the past ages. We are enabled to trace the principle of drainage in the work of our forefathers in surgery by a continual warfare with first one and then another faction in the ascendancy as to the relative advantages of the open and closed method of wound treatment, which was the expression of a sentiment for or against primary wound-drainage. With limited exceptions, the efforts of older surgeons to establish primary drainage partook of the natural variety, content to accomplish their purpose by so shaping incisions as to facilitate discharge or by the practice of a partial or complete open method of treatment. I say this was the method with limited exceptions, for we do find mention of the introduction of pledgets of lint and gauze as a primary procedure, though the purposes and limitation of these measures were poorly understood. As a matter of fact, the

prevailing idea was that it gave an exit for products of inflammation, and it was left in the wound long enough to provoke what it was intended to relieve. To the great discredit of some modern surgeons, traces of this combined primary and secondary drainage, or perversion of drainage, is still to be found at the present day. Interesting and instructive as it would be to trace the great drainage principle through the network of advances in past surgical progress, it is not direct and practical enough for our present purpose. The subject of primary wound-drainage may be said to be under very active discussion at the present time. It is true the argument is, in some respects, a subdued one, for men naturally hesitate to take sides against a measure which every surgeon recognizes has been second to none in saving human life; yet, within the past two years, and especially within the past twelve months, we have all seen Journal and Society reports containing long lists of operations, all with the ever-inserted lines, "Closed without drainage." Converse in private with almost any one of the authors of such a paper, and you will find him more bold, probably admitting that he believes the time has arrived in wound-treatment when primary drainage can be dispensed with entirely. We all know that beneficial as drainage is and has been to our art, yet it is not without its evils; that in its application we either introduce in our wound a foreign substance, or at least an objectionable material; hence, we hail such expressions as prophetic of the time when our ideal is attained; when the brilliancy and achievements of modern surgery have forever reduced the open wound to the benign conditions of a subcutaneous one. Again, in accounting for the facts contained in such assertions as referred to above, we attempt to explain by the different conditions that probably surrounded the work of such men, the nature of the operations performed must have been very different from those encountered by men doing a different line of work, or general surgery. To others such reports tend to create a feeling of dissatisfaction with their own work (a good feeling to have produced at times), probably mingled with the disagreeable thought that they are

not in the front file, but merely high privates in the rear ranks.

After twenty-two years of more or less scientific use of primary artificial drainage in open wounds, we are confronted with the question: Is drainage a necessity; if so, when, and what is the best method of practising it? Reply to the first part of our question involves a brief *résumé* of the principles upon which primary drainage is founded. Such drainage is intended to convey from cavities and recesses of wounds blood and serum. When the physiological time for this has expired the function of primary drainage has ceased. This time, with rare exceptions, is fairly definite, and is between twenty-four and sixty hours. To wait longer is to encourage trouble by the maintenance of a disagreeable guest; to remove sooner than twenty-four hours is taking risks not warranted in the premises. When the surgical conditions are such as to give evidence that its work is accomplished the drain should be removed.

Primary wound secretion is justly accredited with being the nidus or soil upon and in which multiply and develop the noxious agents so inimical to healthy wound repair; hence the limitation of its production, its sterilization, or its removal by some channel from the wounded tissue becomes a surgical necessity based upon the soundest surgical science. If we secure a limitation in the production of serum, or its sterilization *in situ*, is the successful progress of a wound assured without drainage? As to the first, we answer unhesitatingly, yes. The nature and extent of a wound, the character of tissue involved, or both, may be such that with careful aseptic precautions and thorough hemostasis, the production of serum is practically prevented, at least as a factor in disturbed repair; hence drainage is not only superfluous, but detrimental. In the more extensive wounds we have in the buried gut suture a means of the greatest value in enabling us to dispense with drainage, by obliterating spaces, securing compression and rest, and thus preventing pernicious collections of serum. If we ever expect to adopt successful and uniform closing of

large amputation wounds without drainage I feel satisfied it can only be accomplished by means of the deep buried suture which obliterates the deeper parts of such wounds. Other means, while often successful, are not attended with the positive and uniform results which should characterize most modern surgical work. In this connection it may be added that the same principle of deep suture may often be carried out by greater attention to extraneous means of compression by gauze compresses, which, by mutual compression from without, tend to obliterate the internal spaces. As an accessory means of limiting serous exudation must be mentioned the necessity of a more general use of immobilization measures in soft tissue wounds, and thorough attention to systemic or alimentary drainage. Thus when the production of serum can be limited, other things being equal, drainage may be dispensed with.

Second: Will the sterilization of serum *in situ* assure the progress of a wound without disturbance? Let us dwell upon this a moment, for in the elucidation of this point rests the chief answer to the question, Can drainage be dispensed with? To illustrate this point, we will suppose a wound under the following favorable conditions: Perfect sterilization of all tissue adjacent and in lines of continuity solution; perfect sterilization of everything brought in contact with the wound; perfect hemostasis; perfect approximation of skin and superficial structures, and perfect protective dressings. The only undesirable thing in such a hypothetical case as stated is that there are deep spaces and pockets in which serum accumulates. With all the aforesaid favorable protective conditions, it is reasonable to suppose this serum is as inoffending as in a subcutaneous wound. We grant that it may be; indeed, the probability is that it will; however, if in sufficient quantity, its presence will mechanically excite such irritation as may bring into being two latent elements of disturbance. The first of these is to be regarded in the knowledge that no wounded tissue once exposed to air, other than that of the highest mountains, or mid-ocean, is perfectly free from contamina-

tion; it probably has lodged in it pus-forming micro-organisms deposited by the air, yet in such limited quantity that if further ingress is excluded, and not too much and luxuriant soil provided for their development, the vital resistance of the tissues is able to take care of them. Again, it is not safe surgery to tempt the lonely micro-organism in his occasional voyages around the blood-circuit, with large and fertile fields of even sterilized serum, pent up in tissues and cavities of the body. Ever remember, as practical surgeons, that the aspiration or drainage of a *hydrops articuli*, or pleuritic effusion, probably saves your patient a future purulent accumulation with all its disastrous attendants. Then, tension by irritation may convert an accumulation of serum into a dangerous wound disturbance, calling into activity latent micro-organisms which would otherwise have been disposed of by the tissues. Last, in this connection, must not be forgotten the fact that primary wound secretion, though so protected as to be a barren soil for the growth of disturbing agents, may, by its mechanical effects, be fatal to the functional interests of certain wounded structures, notably the brain and cord.

To this, the first part of our query, and one that the idealist in surgery is almost answering in the affirmative, we say no. I have not dwelt upon the greatly differing conditions that exist between the work of the specialist and that of the general surgeon, or more properly the conditions that confront us in accidental surgery in contra-distinction to surgery of selection and timely preparation for. These differences readily suggest themselves to the practical general surgeon who has to deal almost daily with contused and lacerated wounds, the result of machine or railroad accident, where grease, dirt, and foreign bodies constitute no small element in determining the necessity for drainage, if good results are to be expected.

The second part of our question, When shall we drain? has been anticipated pretty largely in the previous discussion of the necessity for drainage. A proper appreciation of the function of drainage, conditions which surround the wound and manner of infliction, the perfectness of our appliances,

and the constitution of the patient will be the chief determining factors. The axiom in abdominal surgery, "When in doubt, drain," is a most excellent one in general surgical practice, unless, like some surgeons I have known in abdominal work, always doubting. Probably we shall best know when to drain by formulating rules when not to drain. If the deeper parts of a wound can be approximated; if pockets and recesses can be obliterated by suture or compression; if the conditions favor natural drainage, dispense with drains. The buried suture will eliminate in a scientific way more drainage-tubes than any measure we are now acquainted with.

How shall we drain? Shall it be tubular or capillary? With material of an absorbable kind, or material to be removed from the wound after its function of drainage has been performed? To answer these questions, let us again think of the purposes of primary drainage, and we will see that a failure upon part of the profession to appreciate the best forms of drainage is due to the want of knowledge in its management. If the surgeon knows how to manage drainage when once established, he will not be long in discovering the best material, at least for his own use. As between tubular and capillary drains, in other than wounds of cavities of the body, it seems the preference should always be given to those acting by capillarity. With a thorough appreciation of the time required for primary drainage, and a knowledge of how the fluids, blood, and serum escape from wounds, there does not seem a valid reason why rubber tubes or absorbable tubular drains should ever be used. Certainly, when possible, preference should be given to a material which does not disturb the wound for its removal, also to material which so closely approximates the tissues in its composition as to permit of its softening, liquefaction, and final absorption. Such a substance is found in catgut strands. In large wounds with free secretion, six or eight threads of large gut made to traverse the entire deep part of the wound, projecting from both angles, will drain it more effectually and more certainly than a fenestrated rubber tube one-half inch in diameter. If there is

any difficulty from bowing or fixing just where wanted, the method of Mr. Chiene, in which the strands of gut are tied in a bundle by a strand around the centre, and these with a needle attached sewn into the bottom of the wound, may be practised. Such a drain does not become plugged by blood-clot, and by virtue of its capillary action it will decant fluids from the bottom of pockets where a tubular drain will not. It is painless, for it does not produce compression; it does not soften too soon, hence before its function is fulfiled. No dressings ever have to be disturbed for its removal. It lessens the paraphernalia of the surgical outfit, hence is an element of simplicity which, in turn, is one of the greatest elements of safety in surgical practice. From the same bottle you tie the bleeding vessel, place your drain, approximate the wounded surfaces, and again use as buried suture to prevent drainage. The size of the gut used, as well as the number of strands, should be proportioned to the size of the wound to be drained. Where there is troublesome parenchymatous hemorrhage, or oozing from a large denuded surface, and it is desirable to combine drainage and hemostasis in the same measure, strips of iodoform gauze form our best method of primary drainage. In all other surgery, with the exception of the cavities of the body, the catgut strands are to be preferred.

SUMMARY: 1st. The principle of artificial drainage in surgery, while very ancient, was imperfectly understood, and was oftentimes as much a factor for evil as for good.

2d. Though our knowledge of the principles which govern a healthy regeneration of wounded structures has greatly advanced, and our progress in wound therapeutics kept pace, we fail to appreciate how artificial primary drainage can be altogether dispensed with in surgical practice.

3d. To lessen the use of artificial drainage it is necessary to thoroughly apply the principles of asepsis and antisepsis, combined with buried sutures, fixation, and alimentary or systemic drainage.

4th. Where, from any reason, the production of serum

cannot be controlled, its removal by drainage is a safer surgical measure than any attempt at sterilization *in situ.*

5th. The time required for primary drainage is from twenty-four to sixty hours; to wait longer is to encourage trouble; to remove sooner than twenty-four hours is taking risks not warranted in the premises.

6th. Capillary is to be preferred to tubular drainage in wounds other than those of the large cavities. For this purpose absorbable material should be selected, catgut being the best. The size of the gut and number of strands employed should be proportioned to the size and extent of the wound to be drained. Drainage by rubber tubes should be relegated to secondary or pus drainage.

7th. Where it is desirable to combine hemostasis and drainage in the same measure, the strips of iodoform gauze, as recommended by Mikulicz, fulfil a most useful purpose.

8th. Where natural drainage can be utilized without producing unsightly cicatrices, artificial drainage should be dispensed with. When feasible, combine the two.

9th. Wounds involving the brain and cord had best be drained, to avoid mechanical violence to the function of delicate structures by retained serum.

10th. Necessity for artificial drainage will most often arise in wounds invading the large cavities; here inflexible tubular drains (glass) best meet the requirements, aided or not by materials acting by capillarity.

11th. The method of secondary suture after primary wound secretion is over, advised by Kocher, seems to possess no advantage over drains that have to be removed, and certainly is not to be compared in convenience, comfort, etc., to the patient, to absorbable capillary drains.

DISCUSSION.

Dr. Howard A. Kelly, of Baltimore.—I would like briefly to make a positive statement regarding my position relative to drainage in abdominal surgery. I consider drainage a confession of imperfect surgery, an acknowledgment of one of two things:

either we have introduced asepsis which we try to get rid of at the end of the operation by inserting a drainage-tube, or the operation has been such that we have not been able to eliminate all those septic materials which we found in the abdomen. Do not understand me to say that we can do ideal surgical work in all cases, and thus do without drainage; but where our work is not ideal in the way of eliminating septic troubles, then we have recourse to the drain.

The drainage-tube is, however, used far more than it ought to be. If operators would be more careful in points of detail, in checking hemorrhage, in cleaning up the field, they would want the drain but seldom. I formerly used to drain anywhere from 75 to 85 per cent. of my cases; to-day I am draining about 10 or 15 per cent.; my comment is that I am doing better surgery than I did three years ago.

Careful bacteriological studies of the fluids in the tube as it lies in the abdomen will be found in the *Johns Hopkins Hospital Reports on Gynecology*, showing the disadvantage of drainage and giving explicit directions how to manipulate the drainage-tube aseptically.

Remember, always, when you open the abdominal cavity and put in a tube, you practically leave the abdomen open, and you are bound to treat the tube tract afterward with the same care to avoid sepsis as you did the abdomen when you took a tumor out, for careful bacteriological studies in my clinic at the Johns Hopkins Hospital show that the drainage-tube becomes infected from the surface after the second or third day with the white skin staphylococcus. But few of these cases suppurate; but the fact remains that the tract becomes infected, and suppuration sometimes occurs. To avoid contaminating the tube with the fingers, I require my assistants to use rubber gloves or finger stalls in handling the drainage-tube, and to clean it out with specially devised forceps with sterilized cotton, so that the treatment is as careful, as far as sepsis is concerned, as the treatment of the wound during operation.

The better way to drain is with a gauze bag containing several strands of gauze. A glass tube does not drain away all the blood clots, as those gentlemen who use the tube so largely fondly imagine. In post-mortem, where the tube had been used, I have found clots scattered all about it and among the intestines. The gauze-bag

drain is flexible, and it *takes care of itself*, and is therefore the best form of drainage for the abdominal cavity. It removes all fluids rapidly, and that which remains is quickly absorbed.

If, now, you will use a drain, do it efficiently. In widespread infection of the peritoneum do not be content with one drainage-tube, or a little gauze, but make two, three, four, or five openings, if necessary, at various points in the abdomen to secure complete drainage, stuff the wound full with gauze, using yards of it if necessary; in this way some desperate septic cases will be saved. Drain but seldom, but when you do drain do it thoroughly.

Dr. Hunter McGuire, of Richmond, Va.—I am just as well convinced that without drainage sometimes you lose patients after laparotomy as I am of any other thing connected with that operation. The day before I left home I removed a multilocular ovarian cyst, one portion of it being down in front of the rectum, filling up the lower pelvis. On stripping it loose from the rectum I found forty points of hemorrhage. I could not wait long enough to tie those forty bleeding points, so I packed with iodoform gauze, and after a short time took that out, and put a drainage-tube in. Without that drainage-tube I am satisfied the patient would have died. As it is, the patient is doing very well. You cannot have ideal surgery except in papers. You may do the nicest laparotomy work you can conceive of, and sometimes you will leave bleeding points. You have also a dropsical condition of the patient where there is an immense amount of serum pouring into the abdomen after the removal of the tumor. What are you going to do with it?

Take appendicitis. Not long ago I opened a child's belly for an abscess from appendicitis. As soon as I found the abscess I took a pair of scissors, made a hole through the loin into the abscess and drained it from behind. I did not let a drop of pus enter the peritoneal cavity. In about a week it was well. Without drainage it would have been impossible to treat it in the way I have said. In ordinary surgery gauze drainage is of immense value. To teach ideal surgery so nicely misleads the younger men. Some of them lose cases which they might have saved by the simple plan of drainage. There is nothing so nice as to put in a few strands of catgut. What harm can it do? It will save some woman, whose breast has been amputated, from a collection of pus down deep by the side of the pectoral muscle.

Would you not drain after amputation of the thigh or the hip-joint, or after resection of the knee? I simply rise to say that I have not reached that point in surgery yet when we can dispense with drainage.

Dr. Henry O. Marcy, of Boston.—My views upon the drainage of wounds are known to most of the members present. It is a large and important subject, justly occupying the attention of the profession as never before. It is necessarily based upon the knowledge which we possess in regard to wounds, their conditions, and the living forces which modify them. I will limit myself to only one or two of the conditions which our friend, Dr. Kelly, has ably outlined to you. I suppose we need not delay over the conditions of septic wounds. The general, well-founded opinion is to drain. Here we have a well-known enemy —a micrococcal infection—and it is plainly our duty to aid nature in her effort to eliminate her enemies. I would only emphasize the advice of Dr. Kelly—when drainage is demanded, make it thorough and effective. This is the more important, since the surrounding structures are usually invaded, and the tissues should be placed in a condition most favorable for the restoration of normal vascularization and improved nutrition. This, however, is not new. Introduced afresh by Chassaignac, it has continued to our day, and, until recently, used in a routine way, with no better knowedge than in the days of the great masters, Petit, Heister, and others equally celebrated. Even gauze drainage, supposed by many to be of a very recent introduction into surgery, was then in general use. One of the most relied upon was parallel fibres of lint, soaked in spirits of wine—an admirable antiseptic.

I have been greatly interested in the discussion of the even more important subject of drainage in aseptic wounds. Against this too general practice I raise an emphatic protest. The lesson which the bacteriologist has taught us is the character of our invisible enemies, their normal development, and the part which they play when introduced into wounds—infection. One of the most interesting and, perhaps, important to the surgeon, of modern discoveries is, that the *micrococcus pyogenes albus* is the normal habitat of the decaying epithelium of the skin—a means of ridding us of a dead superfluous product. These are the common sources of danger to wounds made in septic structures.

They must be removed or destroyed in the neighborhood of the part to be operated on, and especially upon the hand of the operator. Interesting, however, as are these questions, they cannot be entered upon here; indeed, must be presupposed as mastered by every man who hopes to do good modern surgery. Let us accept as demonstrated, that an *aseptic* wound may be made and maintained *aseptic* in *aseptic* structures. It does not require argument to show that such a wound should not be drained, since the only reason for the introduction of foreign bodies into wounds is the hope of removing therefrom deleterious substances more injurious than the material thus employed. The fluids in a wound, if aseptic, are not dangerous, and if the parts are carefully maintained approximated, so that there are no pockets of considerable size for the retention of fluids, the serous or bloody effusions are rapidly utilized for the repair of the wound. All will admit that the portion of a wound separated by a drainage-tube unites only in a secondary manner, and often leaves a weak, imperfect restoration of the parts. In laparotomy, involving aseptic structures, the too common practice of introducing a drainage-tube to the bottom of the pelvic cavity is objectionable and fraught with much danger, even in the hands of the most experienced. Suction of the fluid from the tube must replace it with air from the borders of the wound, and this is rarely, if ever, sterile. If infection, in any way, has entered the peritoneal cavity, its removal by this means is seldom effected, and when peritonitis has once commenced, little good will follow from such imperfect drainage. The use of the drainage-tubes in aseptic wounds may, indeed, be accepted as evidence, on the part of the surgeon using them, of a lack of confidence in his ability to make and maintain the wound aseptic. It has been wisely said by some philosopher that there are no *little* things pertaining to life. It is certainly true in surgery. The technical training of the *modus operandi* of wound-treatment must be mastered as the ritual service of a higher religion. The greatest care must be exercised, not only in avoiding the introduction of the bacterial *seed*, but the wound proper, the *soil*, must be cared for with equal thoughtfulness. As far as possible, tissues necessarily devitalized should be removed, the structures evenly coapted, and the different layers of the tissues united with as little injury as possible, leaving a clean, dry wound. The best methods of

effecting this are still open to discussion. Sponging should be minimized. Coaptation of the different structures, effected by means of fine, continuous suturing, appears to me to possess very many advantages. For the last six or eight years it has been my almost undeviating practice to close the skin also by a line of running tendon sutures, taken through the deep layer of the skin only, which evenly coaptate it, and the suture is thus buried beneath the surface. This is sealed by iodoform collodion, reinforced by a few fibres of absorbent cotton, which remain until loosened by the proliferation of the dying epithelium. My own experience with the use of weak sublimate irrigating fluids, used as far as possible as a substitute for sponging, has been so satisfactory that I am by no means prepared to abandon it because of certain well-founded theoretic objections.

In a somewhat recent discussion with Mr. Lister upon the subject of the drainage of wounds, he stated that he used the drainage-tube far less often than formerly, less in number, shorter, and removed earlier from the wound. I am assured that a better knowledge of the conditions of wounds will restrict the use of the drainage-tube, almost without exception, to those which are septic, and that operative wounds in aseptic tissues will be aseptically maintained without drainage.

Dr. Joseph Eastman, of Indianapolis.—I am not very happy in being constrained to speak after my distinguished friend, Dr. Marcy, who has so ably discussed the question of drainage. But I cannot permit the statement to go unchallenged that we must always know *why* we drain, *when* to drain, and *how*. I agree with the statement as made by Dr. McGuire, that we can only have ideal surgery in papers. If we could have ideal patients we could always have ideal surgery. Why is it that the name of Dr. Dudley has gone down in history, and is so well-known in connection with the surgery of the South? Because he kept his patients so long under observation before making operations. It was a study of the patient, the digestion, secretions and excretions, to obtain the knowledge as to whether he had an ideal patient to deal with, or one whose organization was in some manner defective.

Furthermore, if we could always have good atmospheric conditions it would be very different. We did not have good surgical atmosphere during the prevalence of la grippe and

peritonitis, which have seemed to be more frequent during the last year or so. Atmospheric conditions are not always the same during the entire year, nor during different years and different periods when epidemics prevail. We are, therefore, compelled to believe that we cannot have ideal patients, and cannot always know *when* and *how* to drain. This being the case, had we not better give the case the benefit of a doubt and use abdominal drainage?

Dr. Savage, of Birmingham, England, said to me, "I drain in every case. I use a tube not quite as large as my little finger, and allow it to remain in from twenty-four to forty-eight hours. No harm is done."

Notwithstanding the wonderful investigations of Dr. Kelly, which will undoubtedly help us materially in the entire matter of abdominal surgery, so far as drainage is concerned, we have not advanced a step beyond the efforts of the immortal Ephraim McDowell, in letting the capillary ligatures hang out of the wound as drainage. As expressed by Dr. Cartledge, the tube becomes canulated. The ligatures do the same, and we have just as perfect drainage as we wish in this way. While I do not drain in every case, and while I heard Mr. Tait say the more he operated the more he drained, I do believe that until we can have ideal patients, ideal atmospheric conditions, and know there is not some organic lesion of important organs, we had better give the patient the benefit of the doubt, and use abdominal drainage.

Dr. Charles A. L. Reed, of Cincinnati.—This is a discussion the temptation of which I cannot resist. I believe the question of drainage is indeed the essential one of abdominal surgery, whether we decide it *pro* or *con.* I am on the *pro* side of the question. The patients that I lose are the ones I do not drain. The patients that I save are the ones that I do drain. I drain in every case in which I am sure that the drainage-tube can be properly attended to. I have refrained from draining twice in the last year under circumstances that I thought would give my patients the benefit of a doubt. They were comparatively simple appendage cases, operated upon away from my hospital under unfavorable surroundings and under unskilled attention. Both of these cases died from secondary hemorrhage, an accident that would not have gone to a fatal issue had a

drainage-tube been inserted, so that there might have been prompt and accurate detection of the condition that was going on in the inside. If infection of the drainage-tube tract takes place in every case, as has been asserted here, I am forced to the conclusion that such infection must depend upon climatic conditions. It does not occur in Cincinnati. My drainage-tube tracts heal with promptness and accuracy, union being by first intention, as does any other portion of the abdominal incision. If infection be consistent with such results, I confess I am indifferent to infection. In drainage I use a glass tube that has been made clean and sterilized, and I use it to get the fluid out of the abdomen. I do not mop it out by dangerous pledgets of cotton. I do not resort to expedients other than those which have yielded the best results from this method of practice. By the use of such drainage I do remove fluid and clots—clots that could not be removed by any possible arrangement through the employment of gauze.

I concur heartily with Dr. Marcy in the statement that given an aseptic wound, with an aseptic field of operation, we can maintain it aseptic if we use drainage. We can relieve the peritoneal stomata of the task of absorbing this fluid, and thus relieve the patient of the necessity of high temperature. I believe we ought to have some definite indication for drainage, and that indication always exists in my surgery. I endeavor to ligate with care; I endeavor to stop hemorrhage as far as may be consistent with the safety of operation before closing the wound. I endeavor to leave my patient in as perfect condition as possible after operation, and I believe I come as nearly completing my operations as gentlemen whom I have had the opportunity to observe in their abdominal work. What are the post-operative changes possible to a patient over which you have no control by any method of complete surgery? The softened tissue of the pedicle has yielded to the pressure of the ligature, and I have had secondary hemorrhage, as demonstrated in one of my fatal cases by post-mortem examination. I want to say that I have never yet had occasion to study the existence or non-existence of clots in the peritoneal cavity post-mortem when I have used a drainage-tube. The only clots I have found have been the post-mortem clots that have occurred in the cases in which I did not drain.

Dr. George A. Baxter, of Chattanooga.—I am very much interested in the question of drainage, and I want to know what

gentleman in this Association would dare to close a hip-joint amputation and not put in a drain? In another condition we have a necessity or use for drainage that has not been mentioned, namely, in tuberculous cavities, which do not belong to the laparotomist, but to us who practice general surgery. We, as general surgeons, know that there is a possibility of infecting our wounds, and we must take that into consideration in dealing with drainage. The text-books assert that there is occasionally floating within the circulation germs that my lead to the formation of disorganizing changes at different points. I will say—and I know I will be backed up by every general surgeon in the room—that in certain conditions of wound-treatment it is impossible to get along successfully without drainage. I refer to crushing railroad injuries of either the hand or foot. There are conditions where drainage is an absolute, unavoidable necessity. In some of the injuries I have seen in connection with railroad surgery it is an impossibility to secure an aseptic condition of the parts. Dr. Wood, of New York, several years ago advocated an open condition of the wound for some hours until the oozing had entirely subsided and subsequent closure. What was this for? It was for avoiding sepsis produced by the extravasation of serum, avoiding the necessity of pressure in the thorough closure of the wound. It is doubtless a fact that generally, by Dr. Marcy's buried suture, you may get complete approximation; but you cannot always gain complete approximation by simple compression from without upon deep muscular wounds, and you will have sepsis at times in dealing with this class of cases. You will have an outpouring of serum, that from some cause or other, as a contamination from the air, or within, will develop into sepsis and prevent immediate union of the wound by keeping apart those tissues which should have been brought into complete apposition. Whatever the laparotomist may do, for the general surgeon there is no escape from drainage in certain cases, and if I had to leave a drainage-tube out in certain cases I would stop practising surgery. It is well to talk and teach the ideal; in practice we dare not forget the possibilities.

Dr. Willis F. Westmoreland, of Atlanta, Ga.—I think every man who practices general surgery, if he is honest, will say that whenever a surgeon has proper and sufficient drainage, and yet has any pus in a wound, it is his fault or that of his assistants.

There is some negligence on one side or the other. In every operation I do I establish perfect drainage. I leave the patient with absolutely no anticipation of infection in any way. I open up the wound if I have a rise of temperature. If you have no nidus there from the secretions which have not been properly drained off, you do not have pus. It is all well enough to talk about aseptic surgery; that is about the nearest we come to it in this country, so far as our work is concerned. As general surgeons, we do not have any aseptic wounds. Where you have proper drainage and have made an operation under favorable circumstances and surroundings, I do not care whether it is in a hovel or a palace, you need not anticipate any pus. Where you operate and have surroundings such as we sometimes encounter in general surgery, where we are going from house to house, aseptic surgery is a theory, and cannot be put into practice. I think if Dr. Kelly will consider this point for a moment he will realize that it is true. In doing general surgical work we do not have the favorable surroundings that Dr. Kelly has, hence his success in operating without resorting to the use of a drainage-tube. If the doctor will take into consideration another point, that the effusion from an abdominal wound is more readily absorbed than in any other portion of the body he will at once see that there is less necessity of drainage in abdominal wounds than in those in general surgery. In brief, I wish to say that I am an advocate of drainage first and last, and efficient drainage. When my cases are thoroughly drained I have absolutely no apprehension of getting union by first intention. After the first dressing I remove the tube, and under the second dressing have complete closure of the wound.

Dr. Edwin Ricketts, of Cincinnati.—According to the views expressed by Drs. Marcy and Kelly, there are a few of us in this hall doing abdominal work who have quite a number of patients walking the streets who have myriads of staphylococci within their abdomens. I mean there are cases which have recovered carrying within their abdomen these little animals.

In regard to drainage, I want to cite a case. A lady, sixty-eight years of age, had a solid tumor, weighing nearly twenty pounds, which I removed last May from the abdominal cavity. There was quite an amount of fluid. A drainage-tube remained in for one week, yet we continued to draw fluid from it, which

is contrary to the teaching we have heard to-day—namely, that within forty-eight hours the drainage-tube is walled off and no drainage can come through it.

I operated on a case last Monday, in which I used all possible precautionary measures, and, in regard to doing the work thoroughly, I could not detect any fecal odor at the time of the operation, and for that reason I dropped the pedicle and adjusted my drainage-tube; and he who pulls up the fundus of the uterus and adjusts a drainage-tube into the cul-de-sac, and lets the uterus fall upon that tube, will have pressure upon the cul-de-sac at the bottom. No drainage-tube should be adjusted in that way. We cannot lay too much stress on inserting a drainage-tube well into the cul-de-sac. Within eight hours after that operation there was a discharge of gas with contents of the intestines through the tube. I ordered it shortened. I received a telegram last night, saying that in less than forty-eight hours after there was no fecal discharge pouring through the drainage-tube. Had I closed that abdomen without the drainage-tube I am sure I should have been in error. And I take issue with the assertion made in this hall to-day, that the use of a drainage-tube is an acknowledgment of imperfect work.

Dr. William H. Myers, of Fort Wayne, Ind.—I have an expression on the subject of drainage. I resort to it under all circumstances. I have a letter here from Bantock, of London, that coincides with the majority of those present. It was written in reply to a letter I addressed to him concerning the presence of peritonitis after ovariotomy.

The following letter from Dr. Bantock was then read:

12 Granville Place, Portman Sq., W.
September 21, 1889.

Dear Doctor Myers: I had already started for my holiday before your letter reached this country, and on my return I find it, amongst many others, awaiting me. Of course, it is too late for me to return you an answer for the purpose for which you required it; but I may as well give you my answer to your two questions.

1. I do not regard peritonitis as a frequent cause of death after intra-peritoneal operations. Of course, I am speaking of my own practice. I very seldom see such a thing. At one time,

no doubt, it was a frequent cause of death—perhaps the most frequent—but since the drainage-tube has come into use it has, in my experience, become the least frequent cause. Kidney disease has of late carried off more of my patients than any other condition. Thus, although I lost eight in my last hundred operations—abdominal sections of all kinds—ovarian, uterine, and renal tumors and diseases of the appendages and peritoneum—not one died from peritonitis. Three were in the last stage, and died from exhaustion or shock, three from kidney disease, one from hemorrhage, and one—a case of exploration only—matted intestines, and died in a few hours.

2. When, however, a patient dies from peritonitis, I think it may be safely called a case of septicæmia. This may be due to direct infection at the time of operation, or to the leaving within the peritoneum more than it can dispose of—in the form of bloody serum, etc. It is in the removal of this latter cause of death that the drainage-tube has rendered such signal service.

Believe me to remain, yours sincerely,

George Granville Bantock.

Dr. Bantock gives his reasons why he uses the drainage-tube because the peritonitis present in cases after ovariotomy is always septicæmic. That is the object, as I understand it, why a drainage-tube should be used. It is in the removal of the soil and media that the drainage-tube has rendered me such signal service. The letter I have read comes from a high authority, from a man who has had a vast experience, and had not the drainage-tube been used the results would have heen more fatal.

Dr. J. H. McIntyre, of St. Louis.—The question of drainage has been so thoroughly discussed that I may be pardoned if I do not occupy more than a minute or two of your time. I cannot allow this occasion to pass without putting myself on record decidedly and positively in favor of drainage. In my abdominal work where I have lost patients, I find in almost every instance it has been where I did not drain. For the life of me, I cannot see how a drainage-tube can do any harm, even though it really should not be indicated; therefore, it has been my practice for a number of years, "when in doubt, to drain."

Dr. Myers read a letter from Dr. Bantock. It was from him

(Dr. Bantock) that I got my instruction in regard to the drainage-tube. When Lawson Tait, of England; Price, of Philadelphia; Eastman, of Indiana; Reed, of Cincinnati, and others get such fine results where drainage is resorted to, I do not know how we are going to get over the fact that it gives the best results. I would feel it a criminal act if I did not use a drainage-tube where pus was allowed to drip into the abdomen. Wherever you need to irrigate it strikes me you also need a drainage-tube. In an interview with Keith, of Edinburgh, he showed a large jar as tall as I indicate here (about two feet high), three-quarters filled with red serum, showing that the peritoneum under these circumstances would have been water-logged, and the patient would have undoubtedly died. In view of the fact that the best results are obtained in cases where a drainage-tube is used, it is undoubtedly the part of prudence to use it. I have yet to see a ventral hernia where I could truly and with propriety charge it to the drainage-tube. In my own experience I find ventral wounds heal just as kindly where a drainage-tube has been used as where it has not been used. But I may have been fortunate in that regard. I would regret to do abdominal work without a drainage-tube.

Dr. W. E. B. Davis, of Birmingham, Ala.—I do not feel that it is prudent to prolong this discussion. There are a few words I want to say in regard to the use of drainage. I have heard a great many discussions on drainage, and it amounts to a vote of the Association as to who is in favor of it and who is against it, and we are generally as far from a conclusion when we get through as we were when we began. It is impossible to settle this question. A drainage-tube removes from the wound that soil which is favorable for the development of germs. If we have a dry field they cannot develop. A few germs in a wound will do very little harm if the wound is kept dry. On the other hand, a small number would cause pus if we had a favorable soil for their development. The question of drainage in abdominal surgery cannot be settled, because there are men, careful in their operations, who do not use drainage and who get good results, because the peritoneum is capable of taking up fluids and germs that are present, and thus drains the cavity; therefore, with or without a drainage-tube we should get favorable results

in the peritoneal cavity. I think, in general surgery drainage is more necessary than in abdominal work. So long as we have surgeons who do not use drainage and others who do we will have these discussions, and have good results reported from both sides. The man who seldom uses a drainage-tube generally cleanses the cavity more thoroughly; his operation is longer, but he gets the parts cleaner than the man who uses a tube.

The results of Tait, Price, Bantock, and others show that the drainage-tube is not so dangerous as those who do not use it would have us believe. Those who do not use a tube are teaching us to do aseptic operations. On the other hand, those who are using the tube are teaching us how to get as little disturbance as possible from its presence. With a small tube there is little disturbance in the wound. Those who seldom use drainage, generally when they do use it, let the tube stay in too long.

I would like to speak of iodoform gauze. I do not see how we can do away with it in the drainage of abscesses where the abscess is not attached to the abdominal wall. If you pack with gauze in such cases, and in two or three days remove it, the abdominal cavity will be thoroughly cut off from the abscess.

Dr. James T. Jelks, of Hot Springs, Ark.—I shall be brief in discussing the paper of Dr. Cartledge. He spoke of closing dead spaces, the object of which is to prevent the accumulation of serum or the formation of a nidus for the cultivation and growth of bacteria. If we infect the wound—it is a dry wound, it is not dangerous to the patient. You cannot close dead spaces in the pelvic cavity. It is out of the question to close this pelvic wall. There should be no question about drainage, even if we say we do not believe in it. We forget another fact: We have two processes of drainage in spite of those men who profess not to believe in it. We have the peritoneal cavity covered and filled with the open mouths of lymphatic vessels, and they make drainage for us by the process of absorption. In addition to these we have the intestinal canal, which is a drainage as well as a sewerage system.

Some of the gentlemen have spoken about ideal surgery. I take it that we are here as members of this Association, not for ideal surgery in the sense of an ideal operation, an ideal atmosphere, and an ideal patient. We are here as surgeons for life-

saving work. The ideal surgeon is the man who does the most life-saving work, and not the man who makes ideal operations. I do not believe in cast-iron rules in surgery. Our operations should be governed by the indications of the case, and the main indication is to save the life of the patient.

Dr. Cartledge.—I am very much obliged for the extended discussion accorded the paper, and my closing remarks will be brief. I was in hopes that the discussion would confine itself to the principles of drainage in surgery in general, but it has drifted into pelvic and abdominal surgery. What I said in regard to drainage of the abdomen was said in a general way and would govern surgical cavities in general.

The statement was made by Dr. Kelly that tubes do not drain blood-clots. I said in my paper that I thought the rubber drainage-tube should be relegated to secondary or pus cases.

As to Dr. Marcy's remarks, he made the point that we should only drain for a definite purpose. I fully agree with him. The question is, What is that definite purpose? Further on, he says that the purpose should be sepsis; in other words, he confines the use of the drainage-tube to septic cases alone. I cannot agree with any such statement. Where we have a large accumulation of blood and serum, even if it is sterilized, I do not believe it is a harmless product. I believe we do have infection from within, and by the early prevention of mechanical irritation the sources of danger are lessened. I do not think anyone can have an ideal aseptic wound that has once been exposed to atmospheric air. No wound once exposed is ideally aseptic.

As to the infection of the drainage-tube tract, I will grant that Dr. Kelly's observations are correct, yet, if he will take a glass rod, carefully sterilized, and close it within the abdominal tissues, and not allow any nurse to come in contact with it, he will find the staphylococcus there that he speaks of. I believe in the tract of every drainage-tube there are pus-forming organisms. The point is, we overbalance the slight evil of the tube by the great good we accomplish in grave cases. As to the best form of drainage, the main point of my paper was in favor of capillary as opposed to tubular drainage in general surgery. If we ever dispense with drainage in general surgery it will be by the obliteration of the spaces by the deep suture. We cannot do it

in the hip-joint, but I believe if we ever eliminate drainage from large amputations it will be through the buried suture.

One word in regard to Dr. Kelly's first statement, which I make the last. He takes the ground that the use of drainage is an admission or confession of imperfect surgical work. I cannot admit that the use of drainage is an evidence of imperfect work, because I believe there are certain conditions we cannot overcome without it.

THE TREATMENT OF TUBERCULAR PERITONITIS.

By William H. Myers,
Fort Wayne, Ind.

My Uncle Toby considered that the secret of human happiness lay in the possession of a soul-satisfying hobby. If his idea was correct, then it has been our good fortune to be happy since 1821, at which period Drs. Graves and Stokes, of Dublin, first directed attention to the use of opium in the treatment of peritonitis. Afterward, under the teachings of Dr. Clark, it assumed considerable permanence and predominated over all other treatment, and was regarded as curative. This period constituted the opium-habit of the profession. At present we have a progressive invasion by the surgeon of this once purely medical region. Inertia in the profession is passing away. Professors and authors are no longer followed by us with a punctuality equalled only by the old order of monks about penance and absolution. We are just now emerging from the doctrines of the living ancients, that opium, alcohol, and liquid diet is the treatment for peritonitis, whatever its cause, and whether it be general, circumscribed, diffused, septicæmic or tubercular. If there be those present who indorse this treatment under all conditions, then I can only say: "God hath not justly dealt by them," for a correct diognosis, with a correct apprehension of the etiology of each case, must suggest a different line of treatment from that mentioned. We assert that it is only through a perfect diagnosis that we can see when and how therapeutical or surgical measures must be attempted. Upon this all correct treatment

must depend. It is also of primary importance that the etiology be definitely settled at the earliest moment.

"We must learn the beginnings of disease," as remarked by Sir William Gull. "Often when the gathered clouds of the final storm have filled the atmosphere, it is in vain that we look around to see from what part of the heavens it began."

When we have arrived at the conclusion that peritonitis is present, and have discovered the cause, the blow must be struck simultaneously with the onset. No delay can safely be tolerated, the only hope of rescue being the sudden arrest of the disease, for in the language of Hutchinson, it is almost impossible to exaggerate our conception of the wild-fire rapidity with which inflammation of the serous membranes may extend when once an adequate cause has been supplied. By the time that the normal outlines of the abdomen are obscured by tympanitic distention, respiration quickened and shallow, the pulse rapid and wiry, the supreme moment for precise diagnosis is passed.

I need hardly remind you that the greatest advancement made in the surgical treatment of disease was the result of investigations by abdominal surgeons into the tolerance of the peritoneum; this led to new lines of research, and from them have resulted a complete revolution in peritoneal surgery. They soon discovered that idiopathic peritonitis was only found in the text-books and not in the abdomen. Passing from this to the subject of our paper, we may quote the language of Dr. Osler, that "abdominal section for tubercular peritonitis is the most recent triumph of surgery." At the last meeting of the American Association of Obstetricians and Gynecologists I reported two cases of tubercular peritonitis treated by abdominal section, washing out the abdominal cavity and drainage, with complete recovery. Since then I have operated upon another case, apparently with a like result. All these cases were males, the ages ranging from twelve to twenty years. In the last case adhesions were extensive between the bowels, accompanied by tubercular deposits and a strong band of lymph thrown across the colon in the right

iliac fossa, causing obstruction, and was believed to be a case of typhlitis. The diagnosis was only made clear by the laparotomy; there was present only a moderate amount of fluid. The enlargement of the abdomen was due to numerous deposits in the mesentery. The body was emaciated. This case evidently belonged to the adhesive form of the disease, and this accounted for the irregular and impeded peristalsis or intermittent obstruction of the bowels, it being the most prominent symptom. The chief interest in these three cases is the bearing they have upon the treatment of chronic tubercular peritonitis. Let us contrast the result of surgical treatment with medical treatment. Under the latter the prognosis had been so grave as to preclude the possibility of holding out to the patient the least hope of recovery. Loomis, one of Clark's disciples, says: "The treatment of tubercular peritonitis demands small doses of opium, warm anodyne applications, tonics, and cod-liver oil." His prognosis he states as follows: "Tubercular peritonitis, after weeks and months of anæmia and exhaustion, terminates in death." Davis, under the subject "Chronic Peritonitis," says that "all cases arising from tuberculosis are incurable; temporary relief may be obtained by removal of the accumulated fluid by aspiration or tapping, but the diseases which have given rise to the peritoneal trouble, being themselves incurable, there is an inevitable tendency to a fatal termination." Bauer, in *Ziemssen's Cyclopædia*, regards the prognosis as absolutely fatal. The same opinion is expressed in *Pepper's System of Medicine*, and Fagg speaks gloomily of the issue. So, if we trust medical writings on the subject, recovery from this disease must be considered rare and exceptional.

Curability by Operative Treatment.—Koenig, of Göttingen, gives an opinion founded on 131 cases, that by laparotomy 75 per cent. are much benefited, and 25 per cent. completely cured. Manoanga supplies us with sixty-eight cases, of whom thirteen died soon after operation, fifteen disappeared, fourteen remained alive at the end of six months, and twenty-six at the end of twelve months. Homans records two cases

with recovery. Goodell has operated on four cases, three recovered and one died in six months after the operation. Mundé has operated in three cases, with one death and two recoveries. Kelly gives four cases and four recoveries. Imbach mentions five cases on which he had operated, with four recoveries. Greig Smith has operated on two cases, with one death. Pitts has operated on three cases, and all made good recoveries. In a report of 112 cases the result may be stated as follows: One was unrelieved, twenty-six apparently recovered; twenty-eight died soon after the operation; fifty-seven made a more or less complete recovery—nearly 50 per cent.

In support of this new departure I present the following quotation from a recent author: "Though it seems that the surgical claims are extravagant, I nevertheless believe that in the routine treatment of tubercular peritonitis recourse should be had to surgical means, both more frequently and at an earlier period than is commonly the case. Hare, in his recent work, speaking of the two opposing factions in the profession, the one addicted to salines and the other to opium, claims the first-class dogmatically assert that the physician should turn over every case of peritonitis to the surgeon, to be opened, searched, and purged, and he further asserts that too many cases of peritonitis are to-day walking examples of the value of the use of opium to permit any one to assert that this treatment is useless. In reply to this argument, I need only say that "walking examples" are referred to every day to attract the credulous, to crowd the consulting-rooms of charlatans, who trade upon the discoveries of science to palm off their monumental proofs of success.

Diagnosis.—Called to a case, upon what symptoms, objective or subjective, can we rely as authorizing an abdominal section? The presence of tubercular peritonitis may be inferred if pain in the abdomen, resembling colic, and tenderness, often recur with diarrhœa, and febrile excitement present only temporarily, subsidence of these symptoms and the patient relieved, and in a few days again recurring, possibly

in an aggravated form, and accompanied by a mass detected in the umbilical, hypogastric, or iliac regions, the body wasting at the same time, while the abdomen remains large, often tympanitic, and with loss of its suppleness; there may be secreted serum or lymph, and the difficulty of promoting absorption by the usual remedies may be considered. In this discussion we have limited tubercular peritonitis to those cases in which the peritoneal affection predominates—essentially an outbreak of gray miliary tubercles upon the peritoneum, from which inflammation of this membrane ensues. The disease presents itself in one uniform process, but with different stages—the ascitic, the suppurative, and the adhesive. The first is the earliest stage, its product is serum; in the second stage we have pus, the product of the inflammatory action; in the third, adhesions due to the presence of lymph thrown out, the bowels agglutinated thereby, and obstruction resulting therefrom. Now, gentlemen, the treatment must suggest itself to you. In the ascitic form, to remove the fluid, this is essential to recovery; here the trocar and aspirator as curative procedures are abandoned and relegated. I would strongly impress upon you the free use of the knife. This has happily been substituted with the highly satisfactory results already referred to. In the second variety—suppuration—we know of no reason why the same general principles of treatment by surgical interference—the early evacuation of pus—should not be carried out. In the adhesive form, as its name implies, adhesions will be found between the intestines and possibly the parietal peritoneum; here obstruction of the bowels, often incomplete, accompanied by distention is often present. These cases are often latent, and the pathological lesion is only revealed by an operation. The operation needs no especial description, only the suggestion of extreme caution to open the abdomen freely, and to remove fluid lymph and caseous material as completely as possible, and not to depend merely upon the withdrawal of the fluid, the ancient method which is here considered "more honored in the breach than in the observance thereof." I would further advise the avoidance of

germicidal solutions, to use hot water instead, to use drainage, and that the after-dressing and after-treatment be managed in the usual manner.

DISCUSSION.

Dr. J. H. McIntyre, of St. Louis.—The subject of tubercular peritonitis has interested me greatly within the past two or three years. It is only amenable in the encysted variety to surgical treatment. I believe there have been, according to Greig-Smith, one hundred operations within the past twenty years done for tubercular peritonitis. The operation for the relief of this difficulty was stumbled upon by accident. Cases of the disease were mistaken for ovarian cysts, hence section, the mistake not being recognized until the abdomen was opened. I only wish to make a report in regard to the curability of this affection in my own personal experience. My results have been surprisingly good. In these cases a mistake in diagnosis is liable to be made. My last case was in May, wherein I was able to definitely arrive at a correct conclusion, which was borne out by section after the woman was under the influence of the bichloride of methylene. She had been subjected to an examination by four or five competent physicians, who each said she had an ovarian cyst. My diagnosis was tubercular peritonitis, and I did not hesitate to give the patient the benefit of an operation. My practice has been in these cases to open the abdomen, and, by the way, you need to be careful in opening the abdominal parietes on account of the adhesions. You are liable to wound the internal structures. After you cut down upon the sac, wash it out thoroughly with hot water. I use no chemicals in flushing the abdomen under other circumstances, nor do I recommend it in this condition. You should have your water just as hot as the tissue will bear without damage, then use a drainage-tube.

Dr. R. M. Cunningham, of Birmingham, Ala.—I feel that I should not let this subject pass without giving my observations of peritoneal tuberculosis. I have opened the abdomen, post-mortem, some forty times in this disease. I have never done a laparotomy excepting in one case. The medicinal treatment does no good whatever. Aspiration is not often even palliative, as the serum

will reaccumulate in twenty-four to thirty-six hours. I call the attention of the Association to one fact, that many cases of peritoneal tuberculosis are not inflammatory in character. You may have the entire abdomen, peritoneum, and all the organs studded with miliary tubercles. I have seen cases in which there was no symptom of inflammation anywhere. In these cases I found they usually followed diarrhœa.

Another thing: some cases of peritoneal tuberculosis run through their entire history without attention being called to the abdomen either by the physician or patient. We can have this disease, and the physician or patient not know anything about it. We should open the abdomen where there is adhesion and leave it open for a number of days. In the one case in which I operated I left the abdomen open for thirty days, packing and draining with iodoform gauze.

Dr. Cornelius Kollock, of Cheraw, S. C.—My experience is not extensive in tubercular peritonitis. I have had two cases, one in 1889, and the other in 1886. One of the patients was seventy-four years of age—the first case I saw—and I thought that she had an ovarian cyst. The one thing that impressed me was the inflammation present. The tumor was tremulous. Touch it, and it moved the patient's body. I could not make it out at first. When I cut down on it there was a discharge of an immense quantity of serum. I passed my finger between the walls of the abdomen; pressing down a little further there was a jet of pus, and I thought I had struck the cyst. I went on further, washed out all the pus, and found that it was not a cyst, as the abdominal cavity, kidneys, etc., were studded with white spots. The woman recovered. This was the first case I ever had, and was operated on in 1886.

Dr. William H. Wathen, of Louisville.—Did you irrigate the cavity?

Dr. Kollock.—Yes. I washed it out thoroughly with very hot carbolized water.

Dr. Wathen.—Did you leave in a drainage-tube?

Dr. Kollock.—Yes. The wound healed by first intention. The woman has gained some fifteen or twenty pounds in flesh since the operation. The last case I had I diagnosticated as tubercular peritonitis before I operated. Miliary tubercles were deposited all over the abdomen. This patient did not do so well

for quite a while, but finally she got up and is well. It is three years since I operated on her.

Dr. George A. Baxter, of Chattanooga, Tenn.—I would like to call the attention of the Association to one condition in connection with tubercular peritonitis, one that is generally but not always present, and not mentioned in the text-books, namely: there is not only a thickened condition of the peritoneum, but a changed condition of the cellular tissue. It has a grayish appearance, and is distinctive entirely from the whitish appearance of the sub-cellular tissue at this point. I had one case of this character in a negro boy, opened into it, and found the same condition the doctor speaks of, and simply evacuated this fluid. It has been four years since the operation was done, and there is no indication but what the boy has recovered. I have asked the question of a number of surgeons operating for peritonitis in this condition, as to what cured such cases, and invariably I have received the answer, "We do not know what, unless the simple fact of drainage of the fluids contained there accomplishes the purpose."

I will mention anot her case in the practice of one of my *confrères*, where the abdomen was opened for tubercular peritonitis and a drainage-tube inserted, with death following, not from loss of blood, but from the continuous pouring out of serous material. It could not have occurred from anything else. Death occurred simply and solely from the continuous outpouring of serum from the cavity until the patient sank from exhaustion. I have not seen a case like it recorded. The serum simply came from the surface of the peritoneum.

Dr. Kollock.—I have noticed the thickening of the peritoneum referred to by Dr. Baxter.

Dr. Bedford Brown, of Alexandria, Va.—I am satisfied that many cases of supposed tubercular peritonitis are mistaken for other varieties of peritonitis. I am satisfied, furthermore, that peritonitis is a much more common disease than is usually supposed. Localized peritonitis is by no means an uncommon disease, arising from cold, traumatic causes, such as slight blows, injuries, and from the extension, even in cases of dysentery, of inflammatory processes from the muscular to the peritoneal coat. We have peritonitis limited to a portion of the abdominal walls that will give rise perhaps to a little feverishness for a few days,

adhesion takes place, and the fever disappears. If adhesion does not take place and lymph is thrown off, the lymph undergoes a degenerative process which is mistaken for tubercular matter. We may have serum and pus also, and I am confident many cases that have been operated on under the supposition that they were tubercular in character were cases of simple caseous peritonitis.

Dr. Arch Dixon, of Henderson, Ky.—A short time ago I was called to see a woman who had had an abortion five months previously, with septic trouble following it. The abdomen was enormously distended, and there was an effusion of serum. I was asked to see the case, and concluded to tap it. I used a large trocar, and there poured out an immense amount of seropurulent fluid. The latter part of it was nearly all pus. I advised the physician in charge of the case to do a laparotomy and wash out the cavity, and was overruled. The cavity was again refilled.

Dr. Wathen.—The subject of tubercular peritonitis is very interesting, but we are unable to diagnosticate the disease until the abdomen has been opened, because there are a variety of conditions that result in practically the same symptoms; it is then seldom possible to make a positive diagnosis, unless sections of the diseased structure have been carefully examined with the microscope. Hence, in probably the majority of all cases of reported operations for tubercular peritonitis there has been an error in diagnosis. There are a variety of conditions that result in diseases of the peritoneum, local or general, for which laparotomy is as justifiable as in genuine tubercular peritonitis, and I am an advocate of exploratory operations in these cases, because if the operation does not cure the disease, it is practically free from danger if scientifically and aseptically done. I am not prepared to say what effect the operation has in the permanent relief of tubercular peritonitis; but if it cures the disease it is different in its progress or in its relations to treatment from what tubercular disease is in other localities. Many cases of disease of the peritoneum will get well after a laparotomy that would doubtless have died without it. Papillomatous growths on the peritoneum are often malignant, but not always so. In papilloma of the ovary or of an ovarian cystoma, the peritoneum is often studded with papillomatous growths, and

we expect a fatal termination, but after the removal of the cyst or ovary, and irrigation, the patients often recover and remain permanently well. While we are unable to diagnosticate these cases positively, except by a microscopical and bacteriological examination, and do not know whether they are tubercular or caused by something else, we do know that cases are relieved by surgical interference. If the patients are not cured, they are made no worse by an operation, and we ought to give them the benefit of it.

Dr. Myers.—I have but a few words to say. In reply to the remarks of the last gentleman (Dr. Wathen), in which he seems to doubt the cure of tubercular disease, I desire to make a single observation. Speaking of tubercular peritonitis, we classify the cases as ascitic, suppurative, and adhesive. Many cases of the ascitic form are treated with a trocar, and the patients are told that a dropsical effusion is merely a symptom dependent upon disease of the heart, liver, and kidneys. In these cases we ought to use the knife instead, and find out the real nature of the difficulty, and get to the bottom of it.

MODERN BACTERIOLOGICAL RESEARCH IN ITS RELATIONS TO THE SURGERY OF THE GENITO-URINARY ORGANS.

By G. Frank Lydston, M.D.,
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One of the weaknesses of the average specialist is to claim the results of modern scientific research as more important in their relation to his own specialty than to any other part of the domain of medicine and surgery—and I must confess that the preparation of the present paper has been inspired to a certain degree by a sentiment of this kind. I will, however, endeavor not to completely monopolize the developments of modern research for the especial benefit of my particular line of work, but will premise my remarks by stating that in my opinion modern bacteriological and pathological research has nowhere been productive of more scientific and practical progress than in the special field of genito-urinary surgery. It is not my intention to attempt to present even a *résumé* of modern bacteriological research as bearing upon the micro-organisms found in the tissues and secretions—normal and pathological—of the genito-urinary apparatus in various conditions of health and disease, but to speak of a few points which seem to me to be of the greatest practical value. It is not hoped that I will be able to contribute in any considerable degree to the settlement of the question as to whether under certain circumstances the microbial organisms which are constantly to be found in the secretions of the genito-urinary tract are causal factors in pathogenesis of various forms, or, on the other hand, to decide the precise relation of hetero-

genetic organisms to the same processes. The relation between the two, that is, between what may be termed the normal germ and germs of non-pathogenic properties, must certainly be left to the practical micro-biologist.

Under the omnibus term of urinary infection many diverse forms of disease of microbial origin are doubtless embraced. The present state of our knowledge does not admit of any very arbitrary differentiation between the various forms of microbial infection. Suffice it to say, in a general way, that there are many definite pathological changes, and perhaps disturbances of an apparently functional character, affecting the genito-urinary tract, which are either primarily or secondarily due to microbial infection. These processes range in severity from a general infection, which results in effusions and perhaps suppurations of joint-cavities, to so simple a local lesion as a chronic prostatic irritation. In the one the objective signs are both striking and characteristic, while in the other there may be absolutely no objective signs. The conditions, however, are alike due directly or indirectly to microbial action. The diseases which we to-day believe to be of microbial origin were clinically as well known to the surgeons of the olden time as to those of modern days. They too, recognized the existence of certain suppurative conditions of the genito-urinary tract, and appreciated the relations which existed between certain inflammations of the urethra, bladder, ureter, and kidneys. They were familiar with gonorrhœa, stricture, calculous affections, urinary obstructions, and suppression. The results of injury, dietetic excesses, the gouty and rheumatic diatheses, the pressure of the pregnant womb, and the action of various medicaments taken by the mouth, in the production of pathological conditions were perfectly familiar to them. Above all causes of genito-urinary disturbance, the surgeon of the past held exposure to cold and sudden changes of temperature. If we think for a moment we will appreciate the importance of the fact that all of these causes as such have of late years become secondary in many instances to the action of microbes. They are no longer sufficient explanations

in genito-urinary pathology. The pathologists of former days confused the *propter* and the *post* in a most peculiar fashion. The causes which they regarded as principal and primary are now believed to be of secondary importance and merely accessories before the fact.[1] As time went on, pathologists recognized not only local inflammations and suppurations of the genito-urinary organs, but also certain accidents of a constitutional or febrile character, dependent upon genito-urinary disease. These at once assumed a position of importance. It has only been, however, within the last fifty years that even our clinical knowledge of these conditions has been at all clear, and there is much confusion even now in certain quarters as to their etiology. To Velpeau and Civiale are due the credit of the original stimulus for the scientific study of these conditions. To enumerate the various authors who have studied the subject of urinary infection since the point of departure afforded by the work of Velpeau and Civiale would be wearisome. Not to mention, however, the work of Hallé, Guyon, Albarran, and Reginald Harrison, would be unjust to those enthusiastic workers. Guyon has given a very exhaustive bibliography of this subject.[2]

According to Hallé,[3] there are four principal theories of general urinary infections, the most ancient of these being the theory of phlebitis, advanced by Chassaignac and Icard in 1856. This theory implied that the serious accidents which occur in the subjects of genito-urinary disease were the result of a phlebitis of the peri-urethral erectile tissue and the veins about the prostate. This theory consequently implied that all purulent infections secondary to genito-urinary operations were of a purely vascular origin. A second theory is that of nervous shock, advanced by Perrève, Bonnet, Reybard and others. This theory implied a marked depression of vital force due to a nervous shock, the principal causes of which were painful impressions made upon the urethra. A more

[1] T. Rovsing, Die Blasenentzundungen, Berlin, 1890.

[2] Leçons Clin. sur les Mal. des Voies Urin.

[3] Annales des Mal. des Org. Gen.-urin., February, 1892.

recent theory was that of Verneuil, and following him a number of other eminent authorities who ascribed these various conditions to pathological conditions of the kidney. According to these authors the kidney has been in these cases more or less disorganized and functionally aberrated by nephritis, with a resulting defective elimination of the products of retrograde tissue metamorphosis. Under the influence of reflex impressions upon the urinary apparatus, the kidney becomes still further modified by the occurrence of congestion or hyperæmia. Strange to say, a fourth theory is that of Velpeau, and was the first which was formulated. This author claimed that the accidents of a general character which occur in genito-urinary practice were due to the absorption of certain principles of the urine into the circulation. It is certainly a striking fact that this old-time theory was more nearly than any other in accord with modern views upon this subject. A number of Velpeau's pupils, such as Civiale, and more recently Gosselin, believed that the urine itself was absorbed at injured or pathologically altered points in the mucous membrane, and caused urinary infection by a pathological alteration of the blood. That such absorption is possible in pathological conditions was early shown by the experience and experiments of numerous observers. It is interesting to know that the truth regarding urinary infection was within the grasp of these surgeons of former days, and only escaped them because of their ignorance of bacteriology. The fact of urinary infection then, was well known to Velpeau; the cause of the toxicity of the urine remained to be shown by modern research. Can we, in the light of recent investigations, overthrow entirely any one of the four theories which I have enumerated? There is, it seems to me, an attempt to ignore all of them with the exception of urinary infection. This I do not believe to be logical or founded upon sound pathological knowledge. Does phlebitis ever constitute the starting-point of general infection from operations upon lesions of the genito-urinary tract? It would seem that it may do so in certain instances. It is not to be denied that septic pro-

cesses of a general character may take their point of departure in phlebitis in other situations. The fault lay, as I have already remarked, not in the direction of pathological deductions, excepting in so far as the theory of phlebitis was made to apply to all genito-urinary infections of a secondary character, but in ignorance of pathogenic organisms, which were the *fons et origo mali* of the phlebitis. Can we cast aside the nervous theory of the origin of general infection from the genito-urinary tract? I do not believe that we can, for it is my humble opinion that the influence of the nervous system is intimately interwoven with even the theory of urinary toxæmia. We see cases of so-called urethral or urinary fever in which the point of departure is not the action of pathogenic micro-organisms, but a distinct and positive nervous shock. A case, for example, experiences a chill while undergoing the simple operation of urethral exploration; or perhaps a chill comes on a few minutes after the operation is completed. This may pass off, or may be followed by a succession of chills, and may perhaps be followed by fever and sweating. Can the theory of the absorption of toxic principles from the site of the urethral lesion always explain such cases? There may be those present who believe that it can. If so, I would like a rational explanation of those cases of chill that occur in operating upon a urethra which has never been the subject of gonorrhœa, stricture, traumatism, or any variety of infection. Furthermore—and this involves to a certain extent the theory of renal disease as an explanation of urinary fever—case after case has been reported where a simple operation upon the urethra has been followed immediately by convulsions and death, or by suppression of urine with a fatal result at a period more or less remote from the time of operation.

Personally, I firmly believe in the relation of perverted tissue metabolism from nervous impressions as a cause of urinary toxæmia. Recent observations in the chemistry of the urine go far to support me in this view, in which I believe I had the honor of being a pioneer, as it was suggested

in my lectures over ten years ago. Féré[1] and Voisin,[2] in a series of careful experiments, have demonstrated that a ptomaine is formed in the urine of epileptics during the convulsions, which, inoculated into various mammals, produce convulsions of an epileptoid character. These observations are strikingly suggestive, to say the least.

The subject of autogenesis in its relations to the development of certain constitutional diseases demands more attention than is usually accorded it, and it is probable that physio-chemical researches in this direction will, in the future, shed more light upon diseases the etiology of which is now obscure. Among the modern writers who have given considerable attention to the morbid results of perverted physiological chemistry, Benjamin Ward Richardson is, perhaps, the most prominent. It will be remembered that it is to the researches of this author that we are indebted for the most widely accepted and most logical theory of the pathology of rheumatism. It is my impression that abnormal tissue metabolism is a quite logical explanation of some of the typical cases of general infection following genito-urinary operations. This perverted physio-chemism may be quite readily brought about by surgical shock, and is especially marked in the glandular tissues. We know quite well that mental emotions of various kinds, and those impressions upon the nervous system which result in the condition that we term shock, may produce marked changes in the physiological secretions of the body, these changes consisting either in an increased or diminished flow or obscure chemical changes in composition. Thus we may have, through various nervous impressions, an increase or decrease in the quantity of saliva, the lacteal secretion, gastro-intestinal secretion of the urine and of the menstrual flow. A familiar illustration of the chemical effect of various emotions upon physiological secretions is the change in the quality of the lacteal secretion,

[1] Progrès Med., October 8, 1892.
[2] Bull. gén. de Therap., October 15, 1892.

induced by extreme anger or grief. This change, although occult and incapable of demonstration by microscopic or chemical research, is most pronounced in its morbid effects upon the child, cholera infantum of a most fatal character being a frequent sequel of the emotion of anger in the mother. Precisely what this change in chemical composition may be is an open question; but it is possibly a species of decomposition which results in the formation of a poison analogous to the tyrotoxicon discovered by Professor Vaughan in impure cow's milk. It is well known that great care is necessary on the part of those who supply milk for the use of infants to prevent fatigue and various sources of excitement in the cows from which it is taken. It is well known, too, that sexual excitement in the cow is productive of marked changes in the milk and renders it unfit for human food. If such change occurs in one secretion, it is highly probable that all the physiological secretions are susceptible to a similar change. It is possible that in the case of the saliva the emotion of anger causes the development of toxic principles in that secretion, which may possibly explain the serious results that are so apt to ensue from the bite of an enraged human being. The difficulty of proving this theory in the present state of our knowledge of physiological chemistry is obvious. In the case of the urine the influence of surgical shock may be inferred to consist in the development, in some instances, of organic poison in that secretion. We may consider this to be hypothetically analogous to the ptomaines and leucomaines discovered by Selmi and Gautier in both dead and living bodies, and which so closely resemble the vegetable alkaloids, nicotine, brucine, and strychnine. The impregnation primarily of the blood and secondarily of the nervous system by the toxin or toxines, whatever it or they may be, may possibly explain the otherwise obscure and mysterious causes of death following the simple introduction of a sound. Reginald Harrison has asserted the possibility of such a development of toxic urinary principles and consequent toxæmia from their absorption.

We must recognize, it seems to me, two possibilities: 1.

The occurrence of chill due to a reflex spasm of the peripheral vessels as a consequence of operations upon the genito-urinary tract. We must also recognize a similar reflex spasm followed by active hyperæmia, and finally congestion of the renal structure from similar causes. In addition to these two conditions, I believe that we must recognize perverted tissue metabolism with or without fever, as a result, pure and simple, of a reflex from the genito-urinary tract. To this I will allude again in connection with urinary infection. I would ask the distinguished gentlemen whom I have the honor of addressing, if they believe that we can, after these considerations, throw over altogether the nervous theory of those serious and often fatal accidents that follow operations upon the genito-urinary organs.

Can we set aside renal disease as a factor in accidents from genito-urinary operation and disease? None will dispute the assertion that we cannot do so. Nephritis as a cause of the fatal result after operations upon these parts, is too well known by the practical surgeon to admit of discussion. It is the principal factor in the causation of a large proportion of cases of so-called urethral or urinary fever.

The relation of organic and functional disturbance of the kidneys to the so-called urethral fever is a most intimate one. There is probably no case of long-standing obstructive disease of the genito-urinary tract which is unaccompanied by functional aberration of the kidneys, and in a large proportion of cases there occurs, later on, actual organic changes in the renal tissues. This condition of affairs is to be anticipated, and should be given serious consideration in every case of chronic urinary disease. The immediate effects of the kidney difficulty may not be marked because of the activity of vicarious elimination by the skin and bowels, the vicarious action of these structures constituting the means by which the system accommodates itself to the imperfect elimination of the constituents of urine. When, as a consequence of operations upon the genito-urinary organs surgical shock is produced, reflex hyperæmia of the renal tissue is quite apt to result.

This causes a strain upon the circulation of the kidney, which in its impaired condition it is not able to withstand, and as a consequence its functions are completely inhibited, with resultant uræmia. To those who are familiar with the physiology of the nervous system in its more intimate relations to visceral functions the association of renal disturbance and reflex irritation are not at all novel. In weighing this question it is necessary to take into consideration the extreme sensitiveness and the abundant sympathetic nervous supply of the genito-urinary tract.

Drs. Arnaud and Butté, in a paper on "Neuropathic Albuminuria," presented to the Parisian Academy, called especial attention to a type of albuminuria characterized by preexisting and existing symptoms of the viscera innervated by the pneumogastric nerve. The irritation of the pneumogastric in these viscera is reflected from the vasomotor system to the kidneys, causing albuminuria. It has been found that renal aberration is very liable to occur from operations in and about certain special regions, notably the abdomen and genito-urinary organs. The intimate relation of the sympathetic ganglia, through their visceral filaments of distribution and their liberality of innervation of the genito-urinary organs, with the nervous supply to these regions is an all-sufficient explanation of this fact. Nowhere are the cerebro-spinal and sympathetic systems more closely associated than in these parts. To sum up the situation by a homely illustration: it is not surprising that all operative manipulations of the genito-urinary tract are liable to concuss, so to speak, the renal nerve supply, *i. e.*, are likely to bring a reflex strain upon the vascular supply, and secondarily upon the nutrition and the kidneys.

I have endeavored to show that, valuable as are certain recent discoveries of the intimate relation of pathogenic organisms to the serious secondary accidents of genito-urinary disease, we are not justified in sweeping away, as if it were rubbish, the clinical observations and pathological deductions of past generations of surgeons, whose eyes, fingers and logical minds

were so productive of valuable material that we can only regret that they were not supplemented by the modern microscope.

The theory of urinary toxæmia is unquestionably the most important of all in its bearings upon the subject of the general accidents of genito-urinary pathology and practice. It is not believed, however, that the composition of the urine, *per se*, or the absorption of that fluid, has anything to do with the resulting pathological processes, but that they are invariably due to an alteration of the composition of the urine through the medium of micro-organisms of various kinds. The precise character of these micro-organisms has by no means been definitely settled. That the ordinary bacteria of decomposition have much to do with it is probably correct, and some modern researches tend to show the existence of a special type of micro-organism in decomposing urine. The particular product of urinary decomposition which possesses general pathogenic properties has elicited much discussion. We no longer believe that it is the ammonia developed in decomposing urine which produces general toxæmia; that it may produce local pathological changes within certain limits is probable, but only in so far as it acts as an irritant of greater or less strength. We have not yet arrived at the point when we can positively state that any particular chemical compound is the cause of the toxæmic conditions resulting from genito-urinary disease, injury, or operation; but we are warranted, I think, in assuming that such compounds are the principal etiological factors in such conditions, and, reasoning by analogy, we are, it seems to me, safe in the inference that they are similar to, if not identical with, the ptomaines and leucomaines of which we hear so much at the present time. The function, then, of microbial organisms in toxæmias of genito-urinary origin would seem to be of an indirect character, it being the products of the micro-organisms and not the germs, *per se*, that produce the difficulty. But there are cases of infection that are unquestionably due to special types of micro-organisms, ordinary pathogenic microbes playing an important rôle.

In considering the relation of septic absorption to the general febrile accidents of genito-urinary practice, it must be remembered that the absorptive power of the vesical mucosa is very slight, while, as experiments have shown, that of the urethral mucous membrane is very great. Absorption by the unbroken urethral mucous membrane readily occurs. Rarely, if ever, does the intact epithelium of the vesical mucosa absorb toxic or other materials even when they have remained in the interior of the bladder for a prolonged period.

There is hardly any question that absorption by the lymphatics bear a very important relation to the septic infection that sometimes results from genito-urinary operations. A striking analogy is found in the metro-lymphangitis which often constitutes a starting-point for general septic infection in the puerperium. Personally, I am inclined to attribute relatively greater importance to absorption by the venous channels than by the lymphatics in some cases of infection from the genito-urinary organs ; still, it must be acknowledged that the septic infection may take its point of departure in a septic lymphangitis. Other things being equal, it would be natural to expect absorption by the veins to result more quickly in general septic infection than in the case of absorption by the lymphatics. One point which it is well to bear in mind, is the fact that absorption by means of the veins is, perhaps, not necessarily followed by a thrombo-phlebitis. Rapid general septic intoxication, with a fatal result, may possibly occur before thrombo-phlebitis has time to develop.

Regarding the presence of a specific type of micro-organism as a cause of urinary infection, a number of varieties of microbes have been observed by various investigators in pathological urine and in surgical lesions of the kidneys. It is not surprising that the great pioneer in bacteriology, Pasteur, should have suspected the truth regarding urinary infection over twenty years ago. As early as 1875 he publicly expressed the opinion that pathogenic microbes accidentally introduced into the bladder were the cause of urinary infection. In a discussion at the Parisian Academy he said at

that time: "If I had the honor to be a surgeon I would never introduce an instrument into a patient's bladder without having observed the most rigid precautions to avoid the introduction of germs from the external atmosphere." The various conditions which are embraced under the term surgical kidney are unquestionably the result of infection from the more external portions of the genito-urinary tract. Ascending nephritis has been shown by Klebs, Virchow, and others to be due to parasitic infection. The microbes, after entering the bladder, ascend along the ureters, infect the pelves of the kidneys, and even penetrate into the secreting structure itself. According to Lancereaux, renal abscesses under such circumstances contain the same microbes that are found in pathological urine. Microbes have been found by many authors in pyelo-nephritis, these organisms being in some instances bacteria, and in others micrococci. The streptococcus pyogenes has been found in conjunction with various forms of microbes, both bacteria and micrococci, the staphylococcus being especially frequent.

A point of interest regarding the relation of bacterial infection to genito-urinary disease is the occurrence of descending infection of the kidney, ureter, and bladder. It has been shown that germs may infect primarily the glandular structure of the kidney, and that subsequently either these germs or their products may infect the pelvis of the kidney, and descending produce infection of the more external portions of the genito-urinary tract; that is to say, a cystitis may result from a primary septic nephritis acquired by infection through the medium of the general circulation. It has been demonstrated that certain circulatory disturbances afford an invitation for germ infection. Thus Heubner ligated temporarily the vesical arteries; he then released the vessels and observed that the influx of blood was followed by coagulation necrosis and thickening of the bladder walls. He then found that if coincidently with the removal of the constriction of the circulation he injected pathogenic organisms into the blood, septic cystitis and gangrene of the vesical mucosa resulted. Guyon

performed similar experiments upon the kidneys with like result, both as regards the effect of the circulatory disturbance and the *locus minoris resistentiæ* afforded to pathogenic organisms subsequently entering the kidney by means of the general circulation.

In 1886 Bumm reported eight cases of puerperal cystitis, in which he found a micrococcus common to all, which, according to Hallé, was probably the staphylococcus aureus. Clado, in 1887, isolated from pathological urine a bacillus which he described as a septic form of bacteria of the bladder. He experimented upon animals with this bacillus and produced, by its introduction into the bladder, cystitis. Injection of the same micro-organism into the peritoneum resulted in the death of the animal. In three cases of urinary fever this author found in two living patients the same bacillus in blood drawn from the liver, and in one autopsy he found in the blood the same organism. Other authors have isolated from pathological urine micro-organisms which, injected into animals, produce nephritis. Hallé, who has written what is possibly the most comprehensive article upon urinary infection, has given some very interesting observations. In 1887 this author published a very interesting case upon which he formulated his theory of urinary infection.[1] The patient affected by impermeable stricture, with an intense cystitis presented, following each attempt at forcible catheterism, violent febrile complications, which finally resulted in death. The purulent urine collected and cultivated during life furnished in a condition of purity the culture of a known liquefied bacterium. At the autopsy this same bacterium was found in a condition of purity in the urine of the renal pelvis, the parenchyma of the kidney, and in miliary abscesses in the renal structure. They were also found in the general blood-circulation and in the liver. This bacterium injected into the peritoneum of a monkey caused speedy death by general infection. The bacterium in this case was subsequently recog-

[1] Hallé: Bull. Soc. Anat., October, 1887.

nized as identical with the bacterium isolated from the urine by Clado. In 1888 Albarran and Hallé published an elaborate bacteriological study of a case of urinary infection observed in the Hospital Necker, and numerous experiments upon animals.[1] These authors observed this bacterium in 47 out of 50 examinations of pathological urine. In 35 urines, studied by culture, 15 contained this bacterium in a condition of purity; in 20 other cases it was associated with other micro-organisms. In 19 autopsies made immediately after death the pelvis of the kidney contained this bacterium in 18 cases. It was found also unassociated with other bacteria in the pus of a case of pyonephrosis removed by incision. It was found in 3 cases of peri-urethral urinary abscess, and in 14 cases of infectious nephritis. In 2 cases of acute febrile infection an early autopsy showed this organism in the blood, the liver and the spleen in a condition of purity. In 6 cases of fatal urinary fever of slow development, culture of the blood in the large vessels made in 4 cases immediately after death, yielded this bacterium in a condition of purity. With this organism the authors produced in the animal cystitis by injection into the bladder after ligature of the veins, and a fatal general infection by inoculation in the serous cavities, localized suppuration by inoculation of the cellular tissue, suppurative pyelo-nephritis with renal abscess by injection into the ureter after ligature. It would be a work of supererogation to present all of the conclusive experiments which were made by these authors. It is interesting to note the marked pyogenic properties of the microbe discovered by Albarran and Hallé in 1890.

Krögius[2] discovered a peculiar microbe in purulent ammoniacal urine. In ten specimens of urine this author isolated in three instances a micro-organism quite different from that described by Albarran and Hallé, both in the method of its formation, its size, and its colorizing properties. The inocula-

[1] Albarran et Hallé: Note sur une Bactérie pyogène et sur son rôle dans l'infection urinaire. Academy of Medicine, August 21, 1888.

[2] Krögius: Societe de Biologie, July 23, 1890.

tion of this bacillus in the cellular tissue, veins, and peritoneum of the rabbit, killed the animal very speedily in some instances; in others at a later period. At the point of inoculation the bacillus produced œdema and gangrene of the cellular tissue, but no suppuration. Sterilized cultures were not so active. This bacillus Krögius termed the *uro-bacillus liquefaciens septicus.* The researches of this author have been supported and confirmed by others since his original discovery.

As illustrative of the many types of germs that have been found in pathological urine taken from twenty-nine cases of cystitis, Rovsing's studies were very interesting. This comprised twelve species of microbes, some varieties with which we are already familiar, such as the bacillus tuberculosis, staphylococcus albus, citreus, and aureus, and new varieties to which he applied the names of streptococcus ureæ pyogenes, cocco-bacillus, ureæ pyogenes, diplococcus ureæ pyogenes, micrococcus ureæ pyogenes flavus, and four other varieties, which, as Hallé remarks, are nothing but the four preceding varieties deprived of their pathogenic properties. Experiments upon animals showed the pathogenic properties of these various microbes. The point of great interest is the fact that recent investigations have shown the pyogenic microbes found in the genito-urinary tract to be practically identical with the bacterium coli commune. Krögius found among seventeen specimens of pathological urine taken from the fluid, in twelve cases an organism which he recognized as identical witb pyogenic bacteria and with the bacterium coli commune. In eleven cases this bacillus was present in pure culture. He found this organism afterward in secondary suppurative processes of the kidney, and also in the parenchyma of the spleen. Its pyogenic properties were proven by experimentation upon animals. It is unnecessary to go into full details confirmatory of the important relation of microbial infection to the local and general toxæmias dependent upon pathological conditions of the genito-urinary apparatus. I should like very much to consider in detail in this paper the various

phases of germ infection as presented in gonorrhœa and its congeners, but I fear that my paper has already taxed the patience of the Society, and I will endeavor to say but a few words in this connection.

That the gonococcus is an important factor in genito-urinary infection, has, I think, been proven beyond the possibility of dispute. That it is so important a factor as Neisser, Bumm, and others assert, is possibly open to question—open to question at least in this respect, that if it be claimed that without the gonococcus there is no infection of the urethral mucous membrane, or of the mucous membrane of the female genitalia, too much is claimed for that micro-organism and too little respect is shown for other micro-organisms which thrive in the female genital tract, and which may under certain circumstances assume pathogenic properties. It is my firm belief that through evolutionary changes, what may be termed the normal micro-organisms of the female generative apparatus may undergo transformations and assume new and pathogenic properties capable of exciting not only urethral inflammation in the male, but under favorable circumstances—such as are afforded by the traumatism incidental to parturition—may infect the female herself. There is one point which has, I presume, puzzled others as well as myself. Feeling confident that gonorrhœa and its congeners have their origin in filthy, unhealthful states of the female genitalia, I am at a loss to account for the origin of the gonococcus. Is there any organism in the female genital apparatus which may undergo transformation and assume the properties which we know to be peculiar to the gonococcus? Or is the starting-point really in the genital apparatus of the male, the female genitalia acting merely as the culture-bed for the production of this special germ? There is something very striking in the close similarity of the gonococcus and the normal urethral coccus. The question has sometimes arisen in my mind whether the gonococcus is not really a derivative of the urethral coccus; in other words, whether the differences which exist between the urethral coccus and the gonococcus, such differences as

variation in their properties of coloration, affinity for pus-cells, and peculiarity of grouping, might not be accounted for by evolutionary changes in the urethral coccus itself? The special properties of the gonococcus would, it seems to me, be no argument against this possibility, for with evolutionary changes of form and the adaptation to a new environment, *i.e.*, a suppurative inflammation of the urethra, it is not illogical to assume that the acquirement of new and apparently specific properties might result. Even in cases of urethritis which are distinctly gonococcal in origin, or rather in which gonococci are present in great numbers, we stand face to face with a mixed infection. The peri-urethral phlegmons and abscesses, the lymphangitis, the prostatic suppurations, the acute cystitis, and acute inflammations of the kidney, and in women the peritonitis which occurs in the course of gonorrhœa, are due, not to infection with the gonococcus *per se*, but to other germs which are associated with it. Gonococci, even in typical gonorrhœa, never exist independently of the common pyogenic microbes. In the joint, muscle, tendon, and serious complications of gonorrhœa other germs than gonococci, or the products of other germs, are responsible for the condition. I do not know of a more typically mixed infection than that of gonorrhœa.

I might say in this connection, that pus from peri-urethral abscesses following gonorrhœa has been inoculated upon the urethral mucous membrane with a negative result. Ehrmann, of Vienna, has introduced pus from an unopened peri-urethral abscess into a blind sac of mucous membrane in a healthy hypospadiac without effect. The introduction of the same pus into the urethra of the same individual caused only a slight follicular inflammation that disappeared in a few days.

If the non-existence of a specific gonorrhœal cystitis be established it will certainly greatly modify the existing views regarding this much dreaded complication of gonorrhœa. Even prior to the discovery of the gonococcus it was supposed that the gonorrhœal cystitis was due to specific infection, *i.e.*, to the specific poison of the gonorrhœal process. Since the

discovery of the gonococcus it has been supposed that this germ was the exciting cause of a complicating cystitis. Dumesnil[1] denies that there is such a thing as specific gonorrhœal cystitis. He claims that when gonococci are found in the urine they are accidental ingrafts upon the infectious process, having entered the bladder along with the purulent products of the urethral inflammation. They are not, according to him, new products developed from true specific inflammation of the vesical mucosa. It is conceivable that in women, urethral or vaginal pus may get into the bladder in this manner; but, as a matter of fact, in my opinion gonorrhœal cystitis is relatively quite rare in women. Dumesnil claims to have determined by recent researches that gonococci produce no alteration in the composition of urine, cystitis with ammoniacal urine never being produced by these germs. He claims, moreover, that the urine either renders the gonococci harmless, or kills them completely.

Regarding the evolutionary theory of the origin of the local venereal diseases, there is one fact which seems to me to be of paramount importance, viz., if we accept the theory of evolution as applying to the higher types of animal and vegetable life, we must necessarily accept it as applying to germ life, the difference being, it seems to me, that while in the case of the animal and vegetable types as we see them at the present day, differentiation has arrived approximately to developmental perfection—differentiation and adaptation to environment is going on in a most marked degree in the case of the lower forms of life; to such a degree that a marked variation in toxic properties, if not in physical form, is naturally to be expected. One thing is certain, if we accept the laws of evolution as applying to the host, *i.e.*, the animal infected, we must, *nolens volens*, accept it as applying to the parasite, *i.e.*, the microbe. We certainly produce evolutionary changes in the germ, at least so far as its vital properties are concerned in an artificial environment of our own creation in

[1] Virchow's Archiv, vol. cxxvi., 1891.

the laboratory, and it would seem that if it were not for the natural law of evolution governing micro-organisms—which law prevails much more powerfully in its natural habitat than in our culture tubes—we would have absolutely no experimental control over such organisms. The only alternative of this theory is, it seems to me, the view of a special creation of perfectly developed and unvaryingly typical forms. This, I believe, is incompatible with the present status of biology. A point of considerable importance bearing upon the multiplicity of forms discovered in various infectious genito-urinary processes by different observers, and bearing more particularly upon the close similarity of several forms of microbe thus discovered, is the fact that the physical contour of a germ is not an accurate criterion of its special qualities of infectiousness, as germs of alleged pathogenic properties of a precisely similar character have been found in several very dissimilar pathological processes, suggesting that a metamorphosis of the germ may occur, by virtue of which it acquires a variation of properties of infectiousness without necessarily undergoing any change in physical conformation.

The members of this Association will understand very readily that it is impossible for me to discuss this particular phase of genito-urinary infection exhaustively in this paper; it alone would be sufficient for an entire paper of considerable length. I even hesitated to touch upon it—fearing I might involve myself in a misunderstanding of my views through inadequate presentation—much as I desire to hear expressions of opinion and a free discussion of the subject from the eminent surgeons who have honored me by their attention.

DISCUSSION.

Dr. Hunter McGuire.—I have listened to Dr. Lydston's paper with intense interest. It is a subject which concerns us all. I want to ask him a question. When you have a case of prolonged retention of urine for hours in a man, and you sud-

denly relieve him, and following that comes cystitis, has modern bacteriology developed any facts regarding that cystitis?

Dr. Lydston.—That brings up the point of indirect infection of the genito-urinary tract through the medium of the circulation. Some interesting experiments have been made for the purpose of showing, in the first place, whether circulatory disturbances existing for any length of time, and consequent disturbance of nutrition, in cases of prolonged retention had anything to do with cystitis. In the second place, whether circulatory disturbances from nutritive changes afford a *locus minoris resistentiæ* for pathogenic microbes. One author ligated the vesical artery, and after some time there was a certain amount of infiltration of the walls of the bladder—a certain amount of what some call "coagulation necrosis"—in quite a number of cases. Guyon did the same thing with reference to the kidney, and it was shown that this process bore the relation of a predisposing cause to urinary infection, by injecting pathogenic microbes into the animal after restoration of the circulation. After injecting the microbes into the circulation there was no disturbance of the bladder and kidneys where the circulation had not been previously disturbed by temporary compression; but in those cases in which temporary compression of the vessels had been made, with resulting coagulation necrosis and infiltration of the walls of the bladder (the mucous membrane of the bladder particularly), the kidney structure and mucous membrane lining the pelvis of the kidney, inoculation with these same microbes was followed by a distinctly infectious pyelitis and infectious cystitis. I think that covers the ground in the question of Dr. McGuire. We must remember that in cases of prolonged retention we have an element of direct infection by inoculation into the circulation following the disturbance of the circulation incidental to prolonged retention. In every one of the bladders in which prolonged retention occurs we have an infectious process. Cystitis does not occur in a case of enlarged prostate or stricture in the absence of microbial organisms. In every one of these cases we have an infection of the structures affected by the circulatory disturbance incidental to prolonged retention. Then, following the retention, comes infection, whether a dirty catheter be used or not. This does not make much difference if infection has already occurred. Fol-

lowing the retention we have not only circulatory disturbance that affords a point of local minor resistance to microbes, but we have the bladder, teeming with microbes. We have a still more direct relation of the infection to the circulatory disturbance, for the bacteria are there, ready to begin work. We must remember one point, viz., in a large proportion of cases of enlarged prostate the patient is aware of the fact that he cannot empty his bladder for months or, perhaps, years before he comes to the surgeon. He has catheterized himself or has had somebody else to do it for him. He has had cystitis, but not until infection occurred. The pathological experience of the old man with prostatic hypertrophy begins coincidently with his first visit to the stove-pipe surgeon with the dirty catheter.

Dr. McGuire.—What is the relation of the gonococcus to cystitis and pyelitis?

Dr. Lydston.—That point was covered in my paper. Experiments have shown that the so-called gonorrhœal cystitis is not gonorrhœal cystitis; that so-called gonorrhœal pyelitis is not gonorrhœal pyelitis—that is, they are not gonorrhœal processes dependent upon the gonococcus. When you meet with cases of suppurating peri-urethral abscess, abscess of the prostate, or cystitis (the inflammation, perhaps, travelling up to the pelvis of the kidney), you will find that these processes are produced, not by the gonococcus, but by secondary mixed infection, with ordinary pathogenic microbes and their products. This has been demonstrated in the case of the bladder. The gonococcus will not live in or upon the mucous membrane of the bladder. The mucous membrane is resistent, and the constant presence of urine is inimical to the vitality of the gonococcus. This has been shown experimentally. Independently of the question of the relation of the gonococcus to cystitis and urethritis, we must remember there must be a difference in susceptibility between the bladder and urethra. If the bladder epithelium is intact, absorption of toxic materials that would be readily absorbed elsewhere does not occur.

Dr. James T. Jelks, of Hot Springs, Ark.—I do not wish to say very much in discussing this subject, as Dr. Lydston, in answering the question of Dr. McGuire, covered a part of the field I desire to discuss. It is a fact that the gonococcus does not very often attack pavement epithelium. We know that the

bladder is lined with pavement epithelium. The gonococcus attacks usually ciliated epithelium. It is a fact, furthermore, that the gonococci lie upon the ciliated epithelium for a very short space of time, and that they multiply beneath it in the lymph-spaces, travel up beneath the epithelial surface through the length of the urethral canal to the bladder, and create a *locus minoris resistentiæ* by causing secondary infection, and hence it follows that there is no such thing as gonorrhœal inflammation of the bladder. You may have gonorrhœal inflammation of the whole urethral canal, but this stops at the bladder. It may continue underneath the surface in the lymph-spaces, but the mucous membrane of the bladder is not infected by the gonococcus; hence it follows, as a matter of course, that all cases of inflammation of the bladder are cases of mixed infection, due and produced by the absorption of pus microbes within the lymphatic canals.

A word with reference to Dr. Lydston's theory of the origin of these venereal diseases. He wants to know if the Lord made gonorrhœa. I believe that there is no fruit without its seed. I believe in the natural world we have no grain without its natural seed, and every seed produces fruit after its kind, and we are so informed by the old Mosaic books. Moses was familiar with gonorrhœa and syphilis. We have traced these venereal infections not only back to Moses's time, but back five thousand years ago, to Chinese history. The Chinese were as familiar with gonorrhœal and syphilitic affections as we are to-day. We have gone further back than that. Men who have been delving into osteology have uncovered in the caves of France bones which antedate all history—bones which are sixty thousand years old—and yet these bones have conveyed unmistakable evidences of syphilis. It is an unquestionable fact that the venereal diseases, gonorrhœa and syphilis, are as old as prostitution, and we know that prostitution is as old as the human race. Where you find evidences of human life, whether it be one or sixty thousand years ago, there have been evidences of venereal diseases; hence, I revert to the first law in nature, that "there is no fruit without its seed." There is no effect without its cause. The Lord made man, and after the Lord made him and the fruits of the field and gardens there came evidences of venereal diseases; long before sixty thousand years ago these diseases

existed. We do not have infection to-day without a cause. We know there is no inflammatory action without infection, and why should not the same rule hold true fifty or a hundred thousand years ago?

Dr. Lydston.—Dr. Jelks suggested a point to me in discussing the subject of gonorrhœal cystitis which is quite practical. There are many cases of so-called gonorrhœal cystitis that are due to the gonococcus. Acute general cystitis occurring in the course of gonorrhœa is rather exceptional. We often have so-called cystitis of the vesical neck, however, which is nothing more nor less than posterior urethritis.

Dr. Jelks makes the same mistake that every one makes in discussing the evolutionary theory of the origin of infectious diseases, in supposing—we are talking of the local venereal infections, leaving syphilis out of the question—that the fact that these local infections were the same in the days of Moses is an argument against their evolution. What I claim is simply this, that gonorrhœa is developing every day about us. It does not get into perfectly healthy people excepting by means of contact. There must, of course, have been some origin for it. I do not believe that disease germs can develop from nothing by spontaneous generation; noxious germs may arise from transformation of innocuous forms. If the Doctor combats the theory of the evolution of infection, he must set aside the theory of evolution in general.

Dr. Jelks.—Can we have evolution from nothing?

Dr. Lydston.—No, sir.

Dr. Jelks.—Then we have a starting-point.

Dr. Lydston.—We do not claim to be able to define that starting-point. I claim this: We must have a germ primarily, which in itself is harmless in all probability, but which by culture in a favorable environment becomes pathogenic. When it arrives at its acme of cultivation it constitutes the so-called specific germ. This, at least, is what I mean by the "specificity" of germs. I do not believe the gonococcus, as we find it, was always the same; but that certain non-specific types of germ in that excellent culture-bed, the female vagina, may develop gonorrhœa under certain circumstances. We see plenty of cases of women who convey both gonorrhœa and chancroid, yet in whom we do not find gonococci. How can we explain

this? Do all these cases of infection in women result from infection primarily by some man? We must have a starting-point, and that starting-point may be a germ which has no pathogenic properties. Pasteur has proved in his institute what can be done by artificial cultivation of germs. He has also shown that the degree of virulency of germs may vary widely. We are reasoning now from the effects of germs and not from their physical properties. We must admit that infectious diseases have two elements, a host and parasite, and that we may have germs that produce varying degrees of virulence of infection. If we apply the law of evolution to the animal or vegetable host we cannot but apply a similar law to the parasite. There are no two sets of fundamental natural laws that govern organic development. If we apply a certain law to the host we must apply a similar law to the parasite. The powerful influence of evolutionary law in its relations to organic life is so universally admitted that I confess I am surprised to note that the majority of physicians fail to apply it to the germs of disease.

OVARIOTOMY IN OLD WOMEN.

By Joseph Taber Johnson, M.D.,
Washington, D. C.

A distinguished abdominal surgeon from across the sea remarked some six years ago that the last thing had been said about ovariotomy, the fair inference being that under his wise teaching and that of others associated in the work this operation had reached such a state of perfection that there remained nothing more to be said.

Questions in regard to the incision, the management of adhesions, injuries to the viscera, the treatment of the pedicle, irrigation and drainage, the best manner of closing the wound and the after-treatment, had all been thoroughly discussed and apparently settled.

These burning and restless questions, however, refuse to remain settled. Already the position of the patient during operation is changed, and that of Trendelenburg adopted by some.

The toilette of the peritoneum so strenuously insisted upon six years ago is now disregarded so far as leaving large quantities of water, bland fluids from tumors ruptured in the process of removal, and blood in the abdominal cavity, in the confident expectation that the peritoneum will absorb them without detriment to the patient. The glass drainage-tube is used less frequently, and gauze packing is taking its place; so that the last word does not seem to have been said yet in regard to this wonderful operation.

It was not very long ago that patients over sixty years of

age were considered too old to withstand the shock of ovariotomy, and in some instances within my own knowledge, and in one case by my advice, a large tumor was not removed simply on account of the patient's advanced age.

With more rapid methods of operating, less handling of the viscera, and less time spent in sponging the abdominal cavity, these aged women have, within the last few years, been safely and successfully operated on. Shock has been prevented by rectal injections of nourishment and stimulants, subcutaneous injections of digitalis and whiskey, hot bottles to feet and along the sides of the body.

In the three cases which I have the honor to report, and which form the basis and excuse for this short paper, I am quite sure prolonged anæsthesia and manipulation within the peritoneal cavity would have proved fatal.

Case I.—In November, 1887, a lady, aged sixty-seven, was sent to my service in Providence Hospital by Dr. W. D. Hughes, with a very large abdominal tumor, which had been diagnosed and treated for everything else but a cyst of the ovary. She came in a wagon in a recumbent position, and was in such a bad condition that her friends expected her to die. In their sad adieux they gave her and us to understand that they were seeing her for the last time in this world.

The consulting staff gave me a reluctant consent to operate, thinking that she would die anyway, and the operation would do little harm. I got her in as good condition as possible, provided all known means of resuscitation, and operated. Fortunately there were few adhesions; the tumor weighed fifty-two pounds, and was one-third solid.

The incision was unusually long, but was quickly made, the cyst tapped, delivered, the pedicle quickly ligated, and the patient taken back to bed in less than half an hour. Rectal enemata of hot beef-tea and whiskey were given at once, and repeated several times during the day. There was little shock, and the plucky old lady made an unusually rapid recovery. She was alive and doing well when I last heard from her.

Case II.—In September, 1889, Dr. Lincoln, of my city, requested me to see an old lady with him, who had been tapped

several times, and whose physicians had informed her that she was too old and feeble to stand another tapping, and nothing more could be done. She had been told from the first that she was too old to withstand the effects of an operation, that medical science could only afford temporary relief by drawing off the fluid. The tumor was never half-emptied, and rapidly refilled. Dr. Lincoln was requested to take charge of her as a general surgeon. He made a correct diagnosis of a multilocular ovarian tumor, and requested me to operate.

She was not in condition to be taken to my private hospital, and I consequently operated in her own room. The tumor weighed sixty-four pounds. The patient was sixty-eight years of age. She made a good, though rather slow recovery, and is to-day holding the position of matron to the hospital of the Soldiers' Home at Hampton, Virginia.

CASE III.—On October 10th of this year, about six weeks ago, in my private sanitarium, I removed an ovarian tumor, weighing fifty-six pounds, from a lady who was sixty-seven years old, but who looked to be a hundred. This tumor had been tapped nine times in the last eighteen months, and fully four hundred pounds of fluid drawn away. The last three tappings had been less than a month apart, and the operation was done ten days from the last tap. This patient had been solemnly informed by several physicians, nurses, and by many friends, that the knife in her case meant sure and speedy death, on account of her advanced age and general debility. She had been especially impressed with the belief that she would never leave the operating-table alive. She and her family, acting upon these assurances, had steadfastly and emphatically refused all surgical aid. My appeals and arguments were listened to with impatience and incredulity, until I was finally looked upon as a blood-thirsty ghoul, and implored never again to mention the blood-curdling subject. I was induced, against many protests, to tap this lady five times, and Dr. Graham drew off the fluid four times.

The patient finally made up her own mind to have the tumor removed, and came to my hospital in October for that purpose. She has disappointed her many friends and physicians by making a splendid recovery. She had no shock from the operation, which was completed inside of twenty minutes. Her only suffering was from wind colic and an abscess in her arm, from the

effects of a hypodermic of whiskey given by Dr. Graham during the operation.

When this good lady realized that she had passed safely through the dreaded peril of the deadly knife she developed a remarkable courage. I think her firm determination to get well was of great service to her. The only anxiety she caused me was on account of a troublesome diarrhœa which, however, she stated had been her enemy for thirty years, and she was fully determined not to give up the ship for so slight a storm when she had just weathered a gale of such magnitude. She went home just a month from the date of the operation, and bids fair to live for many years to come.

As I stated in the beginning, nearly everything has been said which can be said upon this thoroughly discussed subject of ovariotomy, and my only excuse for taking up a moment of the valuable time of this Society, whose excellent programme shows the remarkable number of forty-three papers, is to add my plea in favor of the old ladies, whom some in the profession, and many out of it, still fear to trust to the kindly knife of the abdominal surgeon.

I beg to add to this short and practical paper a table showing the results of ovariotomy upon thirty-eight women over sixty-seven years of age. Twenty-five of this number were between seventy and eighty, and one successful case by Dr. Homans, of Boston, eighty-two years of age. In this table of thirty-eight cases, all over sixty-seven years of age, there were only two deaths—giving a mortality as low as many good operators have obtained in cases taken as they come, young and old, great and small. By way of comparison I have also added a table of twenty-four cases compiled from the record of 1000 ovariotomies performed by Sir Spencer Wells, between the ages of sixty and sixty-eight, in which there were six deaths.

Improved methods, quicker operations, antiseptic technique, and provisions against shock show 38 recent cases between the ages of 67 and 82, with only 2 deaths, against 24 cases, done twenty years ago, between the ages of 60 and 67, with a

record of 6 deaths. These figures demonstrate, in addition to improved technique, the surprising fact that old age is no contra-indication against ovariotomy. Indeed they seem to have endured the strain and shock equally well if not better than an equal number of younger women.

One explanation of this may lie in the fact that in the aged we do not often meet with the broken down septic cases from pus-tubes and ovarian and pelvic abscesses which we find so often before the change of life. Ovarian tumors after the menopause present fewer complications than before.

Operator.	Age.	Result.	Where recorded.
Janvrin,	77	Success.	Amer. Journ. of Obstetrics, 1884, vol. xvii. p. 171.
Bennet,	75	"	Brit. Med. Journ., 1861, vol. ii. p. 532.
Schröder,	80	"	Olhausen, Krankheiten Ovarien, S. 394.
Schröder,	79	"	" " "
Wilcke, of Halle,	77	"	" " "
Fancourt Barnes,	70	"	Prov. Med. Journ., 1888.
Sir Spencer Wells,	70	"	Med.-Chir. Trans., vol. lx. pp. 224–227.
Sir Spencer Wells,	77	Death.	
Thornton,	70	Success.	Med.-Chir. Trans., vol. lxx. pp. 57, 64, 75.
Thornton,	71	"	
Thornton,	70	Incomplete operation; death in 48 hours.	
Bantock,	71	Success.	Med.-Chir. Trans., vol. lxiv. p. 128.
Meredith,	70	"	Med.-Chir. Trans., vol. lxxii. p. 50.
Halliday Croom,	70	Recovery.	Obstet. Trans., Fourth ed., vol. xiv. p. 94.
Lawson Tait,	70	"	Brit. Med. Journ., 1886, vol. i. p. 271.
Skene Keith,	75	Drainage; recovery.	" " " " "
Homans,	82	Recovery.	Brit. Med. Journ., May 3, 1888.
Owens, of Brisbane,	80	"	Brit. Gyn. Journ., vol. iv. p. 88.
Davis,	75	"	Brit. Gyn. Journ., vol. iii. p. 413.
Thomas Keith,	73	"	Brit. Med. Journ., 1878, vol. ii. p. 529.
Holland,	76	.....	Brit. Gyn. Journ., vol. xxvi. p. 179.
Fancourt Barnes,	72	Recovery.	Brit. Gyn. Journ., 1892, vol. xxx. p. 159.
Edis,	81	"	Brit. Gyn. Journ., 1892, vol. xxx. p. 162.
Haywood Smith,	70	"	" " " "
J. Taber Johnson,	67	"	
J. Taber Johnson,	68	"	
J. Taber Johnson,	67	"	
Joseph Price,	70 years and over, 10 cases, all recovered.		

38 cases, with 36 recoveries and 2 deaths.

Table of 24 ovariotomies between the ages of 60 and 68, with 6 deaths, taken from the record of 1000 operations by Sir Spencer Wells.

Age.	Result.	Age.	Result.
61	Recovered.	61	Recovered.
61	Recovered.	63	Died 7th day. (Septicæmia.)
67	Died 5th day.	61	Recovered.
63	Died 2d day.	68	Recovered.
63	Recovered.	61	Recovered.
66	Recovered.	62	Recovered.
64	Recovered.	63	Recovered.
63	Recovered.	65	Died 5th day.
63	Recovered.	68	Recovered.
63	Died 4th day.	68	Died 5th day.
68	Recovered.	63	Recovered.
68	Recovered. (Died two years later of cancer.)	62	Recovered.
	24 cases.		6 deaths.

THE SIMPLE, SEPTIC, TRAUMATIC, AND SPECIFIC FORMS OF CERVICITIS, AND ITS TREATMENT.

By Bedford Brown, M.D.,
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In practice I have found four well-marked and distinct forms of cervicitis, all arising from different causes, and each presenting in certain respects peculiar characteristics and pursuing a different course.

In accordance with the etiology of these four forms of cervicitis I will designate them as the simple, septic, traumatic, and specific.

This classification will tend to a clearer understanding of the nature and treatment of each form. The revelations of investigation will clearly establish the fact that all cases do not arise from a common cause and present features in common. This arrangement of the various forms of our subject will enable us to comprehend more clearly the etiology of each form, its true pathology and appropriate treatment. I am impressed with the opinion that all those pathological phenomena observed in the diseased cervix, as endometritis, hyperæmia, engorgement, hyperplasia, erosion, ulceration, are due originally to inflammatory action as a basis of morbid influence arising either from simple, septic, traumatic, or specific causes, and that we should in our differentiation of these different forms of the disease be fully alive to the importance of these facts as necessary to the election of appropriate methods of treatment.

Simple Cervicitis.—This form of cervicitis invariably originates from non-infectious causes. A very common cause

is the action of exposure to cold during menstruation. It not unfrequently arises from excessive venery in the married and unmarried. I have not unfrequently been called to treat cases in the unmarried and virtuous which I was satisfied arose from ungratified sexual emotion. In the past twelve months I have had under my care two pure and virtuous young females, the subjects of violent and painful cervicitis, who had the misfortune to have been the victims of protracted marital engagements in which both were disappointed. These are delicate questions to discuss, but they exert an important influence on uterine pathology. The cervix uteri is manifestly an erectile organ, which function causes it to be brought into active sympathy with the ovaries and mammæ, and brings it into active and intimate relations with the sexual emotions, and when there is overstrain of the female organs, either from abuse or neglect, this erectile tissue is exceedingly liable to become congested, engorged, inflamed, and to pour forth muco-purulent matter from the natural outlet, the cervical endometrium.

Another and common cause of simple cervicitis is the effect of tight-lacing and the pressure of heavy skirts on the pelvic organs. Every pound of weight and pressure exerted on the abdominal organs causes just that much displacement of the pelvic organs. The normal uterus has about an inch and a fourth of play of motion in the pelvis. Place a woman on her back, insert the finger in the vagina and cause her to cough, and your finger will easily detect this play of motion. On the contrary, place a woman in tight stays, bound around with heavy skirts, in a similar position and perform the same operation, and you will find that the cervix is forced down in the vagina and firmly fixed on the perineum, where it is subjected to constant friction and irritation.

Congestion, inflammation, and hyperplasia are the inevitable results. The very term simple cervicitis denotes that the disease is of simple origin; that its pathology is uncomplicated; that its effects are limited; that it is incapable of conveying infection, and that it exists purely as a local affection.

The symptoms of simple cervicitis in the acute stage are usually pain, sometimes referred to the bladder or vagina, but very often to the lower rectum.

I have had more than twenty cases in which the patients referred the aching, unbearable pain to the rectum, supposing themselves to be affected with hemorrhoids, when an examination of both rectum and vagina revealed a swollen, enlarged, tender, hot cervix pressing down on the sphincter.

I find this class of cases especially often in virgins who have been the subjects of tight-lacing and long engagements. This pain is usually temporarily relieved by the recumbent position and aggravated by the erect. It is frequently so severe as to debar the patient from exercise. In some cases the clinical thermometer inserted into the vagina will indicate 101° of temperature.

Menorrhagia and metrorrhagia I have found in a certain proportion of cases to indicate the existence of cervicitis. There are certain cases of menorrhagia and metrorrhagia that are due alone to simple cervicitis, and when the inflammation and engorgement of the latter are relieved the hemorrhagic symptoms disappear. Internal hemorrhoids in the female are not unfrequently the result of the pressure of a swollen, inflamed cervix on the circulation of the rectum, with its paralyzing effect on the sphincter ani.

Lastly, it is proper to say that there can be no pelvic lymphadenitis, no endometritis, no salpingitis arising from simple cervicitis, and no pelvic peritonitis or abscess. I have found in practice that we rarely meet with simple cervicitis in the acute, but usually in the subacute or chronic form.

Treatment of the simple form. In the past, much importance has been attached to the idea that endocervicitis is the original cause of this affection, and that our principal remedial measures should be addressed to that feature alone. A long experience leads me to believe that this is a mere symptom of a conservative character, or a sequence rather than a cause, and is a mode of depletion adopted by Nature to rid the overloaded and engorged circulation of its redund-

ancy of white corpuscles, in the form of pus, and in this way preventing what would otherwise inevitably become abscess of the cervix. For a long time I attached much importance to the treatment of endocervicitis and purulent discharges until I found by practical experience that it effected but little, and my views were confirmed by the publications on this subject of that eminently practical gynecologist, Dr. Emmet. Now I reverse my method and let Nature go on in her efforts at relief, while I endeavor to act upon the engorged circulation and hyperplasia and inflamed condition of the cervix, with much more satisfactory results and more permanent progress, and I feel sure that I am acting on the cause rather than the results of disease. When purulent, or muco-purulent discharges are in continuous progress from mucous membranes, wherever situated, you rarely find abscess in nearly contiguous tissues. This is Nature's own method of preventing abscess.

In a highly sensitive, tensely engorged and inflamed cervix it is trifling away time and opportunity to begin treatment by means of iodine, carbolic acid, or nitrate of silver. Vascular tension must be relieved and decidedly reduced as a preliminary measure to local application, or much of our labor is lost. Where the active engorgement and inflammatory action have been in part reduced, the way is paved for local treatment, and our progress will be infinitely more rapid. This partial reduction of hyperæmia can only be accomplished by local depletion of the cervical circulation, either sanguineous or serous. I infinitely prefer the latter method. In this practice at the present time I make it a rule to alternate my depletive and astringent treatment every second or third day. The glycerin or boro-glycerin tampon of Sims is introduced and permitted to remain one day. The next day following the cervix will be found much paler and less engorged and somewhat higher in the pelvis, and in a condition to receive the application of Churchill's tincture of iodine. In two days more there may be some return of engorgement, but less inflammation, when the serous depletion by the glycerin tampons will be repeated, and so on to the end. All of

this time, during this alternate treatment, it will be found that the engorgement and inflammation are being steadily reduced, the dimensions of the cervix diminished and the muco-purulent discharge will decline until final restoration. I attach less importance to the warm douche in the treatment of cervical disease than formerly, but now use it mostly for purposes of cleanliness. By this method of treatment I economize time, labor, suffering, and money to the patients—matters of much importance.

In those cases of cervicitis arising from descent of the organ in the pelvis and pressure on the lower rectum, elevation of the entire uterus must be practised to insure success. The glycerine tampon systematically applied will do much in the gradual process of elevation and in the relief of rectal pain.

There is a class of cases of cervicitis, in which the uterus is bound down by adhesions, and the inflamed and swollen cervix rests low down on the perineum, pressing on the reetum and sphincter ani, which cannot be relieved without elevation of the entire organ in the pelvis. The method which I have practised in these cases is a gradual system of elevation by means of graduated pessaries. This method, because of the slow and gradual manner in which it is performed, can cause no evil result whatever. I begin the process of gradual elevation carefully, cautiously, and systematically with a cotton glycerin tampon packed firmly into the cul-de-sac, and reapplied every alternate day for ten days. Then a Hodge or Smith pessary, with the curve corresponding to the sacrum, almost angular, so as to jut up under the cul-de-sac, is inserted. But the pessary in point of dimensions and form must be such as not to give the least inconvenience. Then the process of progressive elevation is commenced and a graduated system of pessaries is resorted to, by inserting every week a pessary an eighth of an inch larger, carrying out the graduated system of treatment by gradual elevation of the organ until the final object is accomplished.

Septic Cervicitis.—The history of this form of cervicitis is far more complicated than the former. It originates only

from septic infection, and becomes in time a source of septic infection to tissues connected by circulatory or lymphatic communication. Its cause is to be found in the contact of putrescent animal matter while passing through the cervical canal, in the form of portions of putrid decomposing placenta, threads of membranes or coagula, the results of regular labor, abortion on menorrhagia. The cervix is supplied with lymphatic vessels which communicate freely with lymphatic vessels and glands in the tissues of the pelvis. Septic matter passing through the cervix induces septic inflammation and vascular engorgement. Then it is through this channel of the cervical and pelvic lymphatics that septic infection is absorbed and transmitted to the lymphatic glands of the pelvis. There it is for a time arrested, and sets up inflammatory action in the form of genuine pelvic lymphadenitis. That usually involves the neighboring cellular tissue in the inflammatory process, and may or may not result in suppuration and abscess in the roof of the pelvis. I am convinced that the cases which we have heretofore designated pelvic cellulitis are really genuine instances of pelvic lymphadenitis.

It is impossible for septic matter to be absorbed into the cervical lymphatics without setting up inflammation of these vessels in the form lymphangitis. Hence septic cervicitis is nothing more or less than cervical lymphangitis. Lymphangitis of the cervix means the continuous generation of septic poison and its transmission through the communicating lymphatic vessels and its lodgment in the lymphatic glands of the pelvis, and the development of pelvic lymphadenitis inducing serious septicæmia and suppuration.

Another channel through which septic infection from this form of cervicitis may reach the system is through the cavity of the uterus and Fallopian tubes, creating suppurative inflammation in its travels.

In the early stages of septic cervicitis complicated with pelvic lymphadenitis, the cervix will be found swollen, tender, and hot to the touch, and there will be found in the roof of the pelvis on one side of the uterus a nodulated, indurated,

tender localized prominence in every respect resembling enlarged glandular structure. This indurated point goes on expanding, as it involves the surrounding tissues in the inflammatory process and œdema, until a large portion of the soft structures, cellular tissue, veins, lymphatic vessels and glands are involved in the morbid process.

There is only one other form of cervicitis from which there is tendency to develop pelvic lymphadenitis, and that is the specific form, to be discussed in a future chapter.

I have seen numerous cases of septic cervicitis following accidental abortion, sometimes with and at other times without pelvic lymphadenitis.

I have seen cases of septic inflammation of the cervix, with copious muco-purulent discharge, intense redness and congestion, complicated with pelvic lymphadenitis lasting many months before final recovery, arising probably from the fact that the lymphatics of the cervix had become diseased. In a case of septic cervicitis under my charge I could detect on one side of the uterus in the roof of the pelvis a distinctly movable indurated enlargement as large as a hazelnut, clearly indicating an enlarged gland, which became a point of radiation for morbid infection of surrounding tissues, followed by suppuration.

Treatment. In treating septic cervicitis the object is not only to arrest inflammatory action and engorgement, but to arrest septic infection in the centre from which it radiates, and at the points infected.

My custom has been, after a thorough douche of hot carbolized water every day, to apply or insert in the vagina a tampon of glycerin and iodoform. Every third day a thorough application of the tincture of iodine is made over the cervix and entire roof of the vagina. The iodine and iodoform are rapidly absorbed by the lymphatics, and carried to the lymphatic glands throughout the pelvis, exerting their potent disinfecting powers on all the pelvic structures. After experimenting with a variety of methods I have found this the best local treatment. Where there is septic fever, or where there

is no fever present, I have prescribed, with good effect, three times a day, a pill composed of one grain of sulphide of calcium, in connection with quinine and cinchonidine. In the more chronic stages I have prescribed the hundredth of a grain of biniodide of mercury, four times daily, as an alterative, with satisfactory results.

THE TRAUMATIC FORM OF CERVICITIS.—As the term implies, this form of cervicitis originates only from injuries sustained by the tissues of the cervix during labor, abortion, or from artificial dilatation by means of instruments.

Inflammation, congestion, and hyperplasia of the tissues of the cervix resulting from wounds caused by labor, abortion, or artificial dilatation, constitute the most common form of the disease. These wounds are found in the form of the lacerated, the contused, and in the form of fissures in the mucous membrane of the cervical canal, sometimes extending from the external to the internal os, and at other times confined to the internal or external os alone, and in depth only extending down through the mucous membrane. But it is essentially a wound, and, like fissures of the anus, is obstinate and difficult to heal. Complete lacerations are perceptible to the eye, and are diagnosed without difficulty. On the contrary, simple fissures in the mucous lining of the cervical canal, or contused wounds, are often entirely concealed within the canal and are difficult of detection. A small crack in the mucous membrane at the verge of the external os will indicate the existence of a fissure but a line in depth. Or it may exist in the cervical canal in a concealed state, or oftener than otherwise at the internal os. When, in exploring for fissure in the canal, the silver probe is passed up there will be experienced a sharp pain as the instrument passes along the line of the fissure. If it is situated alone at the internal os, as soon as the probe touches that point this sharp pain will be felt by the patient, resembling the sudden touching of the nerve of a diseased tooth. In my experience these fissures in the cervical canal are among the most common causes of cervicitis in childbearing women. They form a basis for the maintenance

of inflammation that is exceedingly protracted and obstinate to relieve, because they are concealed and remain an unknown cause of disease.

Contusion of the os and cervix from the pressure exerted in protracted cases of labor constitutes another important cause of cervicitis. The congestion following contusion is often very great and lasting. The os is much enlarged, swollen, livid, and often œdematous. Its circulation is sluggish and the vessels are partly paralyzed, and with great difficulty recover their tone. In searching for the causes of cervicitis few of us take into consideration the possibility of contused wounds of the cervix. These lacerations and fissures either heal by the first or second intention. Many cases of laceration heal by first intention, and give no further inconvenience. The internal and concealed fissure rarely heals in this way, but may heal spontaneously by second intention. As a rule when these wounds remain open they give rise to engorgement and inflammation. I have seen cases of lacerations and fissures where the engorgement and inflammation were so excessive as to produce an amount of swelling of the os that gave to it the form and appearance of a mushroom. This expanded form of the os while the neck remains small is characteristic of this condition.

I have good reason to believe that the existence of these concealed fissures in the cervical canal often escape observation and are the means of perpetuating for years intractable forms of cervicitis, which resist all methods of treatment until the fissure is healed by natural or artificial means, when the local inflammation subsides.

Treatment. In the treatment of traumatic cervicitis we must from the beginning keep in view the practical fact that we have to deal with a granulating, suppurating wound accompanied by hyperæmia and inflammation in more or less degree. And furthermore, the fact that there is a constant mutual action and reaction in progress between the morbid processes in the wound and the surrounding tissues that are calculated to perpetuate the unhealed wound and accompanying

inflammation. For some years I treated all cases of traumatic cervicitis by means of the local application of nitrate of silver in solution varying in strength from ʒi to ʒij per ounce, whether in the form of complete laceration or a mere fissure in the mucous membrane of the canal. In a very large proportion (three-fourths) I was enabled in a reasonable time not only to subdue the inflammation and hyperæmia but to heal the wound.

The applications were made thoroughly over the external and internal cervix every third day, by means of a brush and probe wrapped with cotton. In the canal the applications were made up the entire tract of the fissure. This treatment can be very much facilitated by means of the cotton and glycerin tampon in promoting serous depletion.

I find now that I can heal these wounds and subdue the cervicitis by means of the alternate application of the tincture of iodine and glycerin tampons. But in the case of concealed fissures the iodine must be carried up the entire cervical canal. In illustration of the practical results of this method the history of two cases will be cited here.

Mrs. B., after her second labor, which was a protracted one, in twelve months was found suffering with a uterine affection that completely disabled her. On examination a quadruple laceration of the os was discovered, each laceration extending a full half inch or more. The cervix was highly congested, inflamed, and enormously enlarged, presenting a mushroom appearance. From these granulating wounds poured forth an abundance of purulent discharge with occasional hemorrhages. There was almost constant pain in the vagina and back, and the general health was seriously impaired.

Every second day for ten days the glycerin tampon was inserted, creating each time a copious flow of serum and causing effectual depletion, to the great relief of the local symptoms. Then the alternate treatment of iodine and glycerin tampons was instituted for some two weeks, and finally, when the engorgement and intense inflammatory action were corrected,

the lacerations were effectually healed by means of the mild solution of nitrate of silver.

Mrs. F., the mother of three children, had a protracted abortion at four months. For some twelve or fifteen months she suffered with what was supposed to be uterine disease. On examination a very greatly inflamed and highly congested cervix was found. On one side of the os could be seen a small crack, crease, or fissure not more than a line in depth, extending through the mucous membrane of the cervical canal. A silver probe passed along the line of this fissure up to the internal os gave a sharp lancinating pain the entire distance. Around this little fissure at the external os the tissues were more thickened and indurated than elsewhere. The identical treatment was applied in this as in the former case with equally permanent results.

Wounds of the cervix may exist for years without producing engorgement or inflammation in the surrounding tissue. In that case they rarely give rise to trouble in the form of pain or blenorrhœa. It is the accompanying congestion and inflammation that call the attention of the patient to her condition.

Specific Cervicitis.—We come now to the consideration of the specific form of cervicitis. I am satisfied that a very considerable proportion of the cases of cervicitis that come under our observation arise from specific causes; these cases often have had a protracted existence, and the cause of these may be unsuspected and unknown to us at the time. These cases are due either to gonorrhœa or syphilis. I have found both among the unchaste and chaste married females.

Gonorrhœal Cervicitis. The inflammation of the cervical tissues in this form of disease is exceedingly acute and violent. It not only involves the mucous surface of the cervix and vagina, but involves the submucous and often the interstitial tissues, also causing violent congestion and œdema. The organ is intensely inflammed, congested, and red, and the entire mucous surface of the cervix and os pours forth pus abundantly. This constitutes a differential diagnosis from the

ordinary forms of cervicitis. In all other forms the secretion of pus comes from the cervical canal and os. Diffused suppuration over the entire external surface of the cervix denotes gonorrhœal infection.

The acute attack is almost certain to leave behind a chronic form of cervical hyperæmia and inflammation that is exceedingly unmanageable. The infection from this chronic form is very liable to attack neighboring structures, as the lymphatic system of the pelvis. On one occasion I saw a well-marked case of infection of the lymphatic glands of the pelvis from gonorrhœal cervicitis that had lingered for months. The cervix in the acute stage was intensely inflamed and engorged. After the acute symptoms subsided there remained an enlarged and inflamed cervix, with continuous purulent discharge. Then on one side of the uterus, in the roof of the vagina, there appeared a hard, nodulated, movable prominence, which was evidently an enlarged gland. This continued to increase and extend until a large portion of the surrounding tissues became involved. Then septic fever was developed, then abscess. Here was manifestly a case of gonorrhœal pelvic lymphadenitis. Old cases of cervicitis from gonorrhœal infection are exceedingly intractable and continue to be centres of infection.

I have met with this form of disease in chaste married women, contracted from unfaithful husbands, with the symptoms of intractable cervicitis which had infected the neighboring lymphatic glands of the pelvis and their surrounding tissues, producing an extensive mass of diseased structures, dating back to gonorrhœal infection fifteen years previously.

Treatment. Douches containing the peroxide of hydrogen and bichloride of mercury in appropriate proportions effect good results throughout the treatment. We must remember the fact that we are dealing with a disease the infective properties of which may continue indefinitely. The iodoform and glycerin tampon inserted every second day, and permitted to remain twenty-four hours, is a valuable local remedy. Then the alternate application of a solution of

nitrate of silver ʒss to ʒj over the entire cervix and vagina will do much to correct the infection and inflammation.

Syphilitic Cervicitis. This form of cervicitis is attended with a peculiar form of inflammation and hyperplasia, and as its name implies arises from syphilitic infection, either primary or secondary.

In the cases of syphilitic cervicitis seen by me there was a remarkable paucity of secretion of any kind. In the primary form, after healing of the chancre on the cervix, there remains an inflamed, congested, engorged cervix, with no catarrhal secretion, or, on the contrary, secondary syphilis may attack the cervix, causing a protracted cervicitis, accompanied with a subacute form of inflammation and hyperplasia that continues until the syphilitic disease is cured.

As a means of differentiation of the syphilitic from other forms of the disease, the fact may be stated here that no local or general non-specific methods will cure it. They make no impression on the inflammation or engorgement, and it is immaterial how assiduously they may be applied without the co-operation of mercury or the iodides. As soon as the system begins to feel the influence of these, the cervicitis begins to break down, and all concomitant symptoms subside.

In syphilitic cervicitis the tissues of the cervix are peculiarly hard, indurated, injected, dark-red in color, with erosions on the os, having well-defined edges that might easily be mistaken for harmless non-infectious abrasions. These erosions are liable to heal and return, again and again, without apparent cause. The history of the following cases will serve to illustrate the peculiar characteristics of the disease :

Mrs. I., a young married woman, contracted syphilis from her husband soon after marriage. There were two well-marked chancres on the labia. She was treated for this by the usual method, and the chancres healed apparently without evil result. In the next two years she had two abortions, a dead fœtus in both instances, evidently from syphilitic causes. Twelve months subsequently to the last abortion she came under my care with uterine disease. I found the entire

cervix swollen, very hard, deep dark red in color, giving vent on the slightest irritation to a copious flow of blood; indeed, the menorrhagia from which she suffered was her chief source of annoyance. There were two slight erosions on the os, with somewhat well-defined edges. On each side of the uterus, in the roof of the vagina, there were enlarged lymphatic glands, distinct, movable, round in form, well-defined and tender on pressure. These enlargements could not be mistaken for any other structures than glandular. The appearance of the patient was decidedly cachectic, and but for my previous knowledge of the syphilitic taint of her constitution, and the test of treatment, I should have regarded the case as one of intractable cervicitis. She was treated at first for one month by means of iodine and glycerin tampons without the least improvement. Suspecting then that syphilis was the true cause of the cervicitis and disease of the pelvic glands, the patient was placed on a thorough anti-syphilitic course of treatment with the most satisfactory results. The cervical engorgement, hyperplasia, and inflammation disappeared, and the erosion healed and the pelvic lymphadenitis subsided, and the woman recovered robust health. Here was clearly a case of syphilitic cervicitis and pelvic lymphadenitis as a feature and part of constitutional infection in the secondary stage. I should mention also that the menorrhagic symptoms disappeared, and in twelve months the patient gave birth to a healthy child. It will not be out of place here to suggest the propriety, when treating obstinate and intractable cases of cervicitis, to investigate more closely the nature of the case, to ascertain if there are any enlarged pelvic glands, any erosions on the os, however slight, or any constitutional signs of syphilis, and, in addition, if any suspicious circumstances exist, to bring the patient to the test of treatment. My own experience proves to me that there is syphilitic cervicitis, and that we do meet with it in practice. Three cases of genuine syphilitic cervicitis have up to this time come under my observation.

The second case was that of a young girl of eighteen years.

In this case I found a primary chancre situated on the cervix near the os uteri, which had been in existence for some time previous, as there was in each groin a well-defined bubo. The patient was placed on anti-syphilitic course of treatment, but it was very irregularly and imperfectly carried out, and secondary symptoms developed. The primary chancre healed, but there was left a cervicitis, attended with a highly engorged, inflamed, and indurated tissue that was exceedingly intractable, and did not subside until complete eradication of the syphilitic taint was accomplished.

The third case that came under my care was in a woman of the town who had long been the subject of syphilis. The cervix was very dense and hard, of dark-red complexion, tender to the touch, and highly congested and inflamed. Local remedies exerted no beneficial influence whatever in this case, but complete resolution was finally effected by a long and rigid course of anti-syphilitic treatment, and in addition by iodoform-glycerin tampons.

DISCUSSION.

Dr. T. B. Greenly, of Kentucky.—I have been greatly interested in listening to the reading of Dr. Brown's able paper, and I think we should feel indebted to him for pointing out one cause of cervicitis not thought of by the majority of us—mental or reflex cervicitis, due to disappointment in the matter of marriage. I have never thought of that in treating the disease. I have met with cases of what might be termed traumatic cervicitis, due to excessive venery, tight lacing, forcing down the pelvic organs, causing irritation. We may include in this category abortion as a traumatic cause. These are the main causes that produced the disease that I have been called upon to treat. Practising in the country, I have had but few cases of specific or septic cervicitis. I can emphasize the treatment with glycerin and carbolic acid and Churchill's tincture of iodine. Instead of using a solution of nitrate of silver I have used the pencil, introducing it and holding it there momentarily, then painting the cervix with Churchill's tincture of iodine, then keeping a pledget of cotton applied with glycerin, iodine, or hydrastis

canadensis, or something of that kind that would produce an effusion of serum from the organ.

Dr. James T. Jelks, of Hot Springs, Ark.—In discussing Dr. Brown's paper, I desire to say I believe that all inflammation in a pathological sense is due to infection, and that inflammations of the uterus, its lining membrane, and appendages are no exceptions to this rule. I furthermore believe that inflammation of this organ is due to one of two causes, viz.: infection following labor or miscarriage, infection with one of the pathogenic microbes on this wound surface, just as we get it in any wound; or infection with the gonococcus, this latter being the agent in the majority of cases.

In Bumm's experiments he found that the gonococci, after the first day or two, multiply beneath the epithelial surface, and upon and within the papillary layer. Hence it follows that remedies to cure this class of cases must reach the gonococcus as it thus multiplies. We may do this by surgical means, as with the curette; or by medicinal agents, as caustics which destroy the epithelium; and, again, by remedies which penetrate this surface without destroying it.

Recently I have used with great success remedies of this class. We have three of such remedies, viz.: weak solutions of permanganate of potash, iodine, and pyoktanin. With the latter agent, either in powder or solution, say of four grains to the ounce of water, I have seen some remarkably rapid cures of gonococcal inflammation in women as well as men. Hence in all my cases of inflammation of the female organs of generation, when I suspect the gonococcus, I always use Merck's yellow pyoktanin, warning the patient that the medicine will stain everything it touches.

Dr. J. H. McIntyre, of St. Louis.—I was pleased with the part of Dr. Brown's paper that I had the good fortune to hear. I regret very much that I did not hear it all. I think it is true, and it has been well said, that "syphilis kills its thousands, but gonorrhœa in women kills its tens of thousands."

Dr. Jelks has gone over the ground in regard to gonorrhœa, and the manner in which it acts, and has given us some exceedingly valuable pointers in regard to its treatment. I can heartily indorse his remarks in this regard.

In cases of cervicitis there is not infrequently a concealed or

internal fissure, as mentioned by the essayist, and I find that the principle of dilatation comes in nicely here, just as much so as in cases of divulsion in fissure of the anus. I have had made a series of sounds which can be unscrewed easily, by which the cervix can be sufficiently dilated to bring a medicament in contact with the bottom of the concealed fissure, and when you have so dilated it you can bring a medicament in contact with the traumatism that you would not otherwise be able to reach. The matter of a small tear is one not to be overlooked if one wishes to cure the causes of cervicitis. While I am not an advocate of sewing up every little fissure in the cervix, yet I am satisfied there is often a deposition of a kind of tissue that will produce, sooner or later, "a rebellion of the cells"—a cancer. It is well known that ninety-five per cent. of uterine cancers begin in the cervix, and in the majority of cases implanted upon a lacerated cervix. That alone would be a cogent reason for at least cutting out this tissue and bringing the edges of the wound together. I have seen profound nervous symptoms from cervicitis. A case comes to my mind at present, the wife of a carpenter, who was in a condition of wild maniacal excitement on account of a considerable degree of granular erosion of the cervix. The cervix was split upon the inside, but yielded to treatment. I have seen a number of cases where the excitement bordered on mania, arising from erosions and lacerations of the cervix. I find in my experience that women will often complain of a sympathetic ovarian irritation, ordinarily upon the left, sometimes upon the right side, and occasionally upon both sides, from troubles about the os and cervix. Where I am not able to decide in the first place, by the best investigation that I can make, as to whether or not there is real organic disease in the ovary, and there is pronounced disturbance in the cervix, I cure the latter to determine the question and to shed further light on the condition that may be present in the ovary.

Dr. Brown.—I was glad to hear expressions from Drs. Jelks and McIntyre in regard to their methods of treating concealed fissure of the cervix, and particularly with Dr. Jelks's method of treating it by means of pyoktanin. I have never tried violent dilatation in my cases, not but that I believe it may be suitable to them. I have always been able to heal them without it by the remedies specified in my paper.

SHOCK.

By James Evans, M.D.,
Florence, S. C.

In the severe injuries inflicted upon the body by accident, and in the major operations of surgery, not the least element of danger to life is the condition known as shock, which rapidly supervenes. The torn and lacerated tissues may involve only a small area, the hemorrhage may be inconsiderable in amount, or the limb which has been crushed may, under other circumstances, be removed without any great menace to the life of the individual, yet the shock may be so profound and persistent as to resist all efforts to avert death. The degree of shock is not determined solely by the extent and gravity of the physical injury. Certain idiosyncrasies of constitution, the character of the force which inflicted the injury, and the circumstances under which it occurred, are potent factors in its determination. Individuals of a highly wrought and exquisitely sensitive nervous organization bear pain with far less fortitude, and are more susceptible to shock than those of dull and obtuse intellects and blunted sensibilities. If the agent which has caused the injury has vividly impressed the mind with its great power, and that it is capable of exerting a force irresistible and crushing in its character, the shock is always pronounced. A shell-wound is usually attended by more shock than one from a ball, although there may be no greater destruction of tissue. A similar reason may be assigned for the severity of shock in railroad accidents, and the injuries inflicted by powerful machinery. It is evident, therefore, that the physical injuries are not the

only cause of that assemblage of symptoms which is designated *shock*. The prime factor in its production is the impression made on the mind by the perception of the threatened danger, and the serious consequences which may follow whatever wounds may be inflicted. The condition is not originated by, but precedes the infliction of the injury. The irritation of the nerves of sensation at the point of injury is propagated to the central nervous system, and there likewise originates impulses which, being transmitted to the medulla, start into action the same reflexes, and the state is thus indefinitely intensified and maintained. Fear is capable of producing such a profound degree of shock as sometimes to cause speedy death, and this result frequently takes place even before an inevitably fatal injury is felt. Even in cases in which the body has sustained no injury, the effect is so pronounced and persistent as to cause serious illness, which may continue for many days thereafter. This phase of it is well illustrated in an incident which occurred in the neighboring town of Cheraw. In laying the foundation of a bridge across the Pee Dee River at that place, an immense block of granite weighing over a ton was being lowered into a pit forty-four feet in depth, at the bottom of which was a man who was to direct when it was in proper position. When this huge block of stone was suspended over the pit the cable holding it began to slip, and the man below was warned to crouch in a corner, as it would inevitably fall. The rock did fall, and the man in the pit miraculously escaped without injury, but he was taken out in a perfectly lifeless condition and was exceedingly ill for more than a week.

Every army surgeon has frequently observed soldiers in approaching the battle-field, when within the sound of musketry fire, exhibit an uncontrollable desire to evacuate the bladder and bowels. The apprehension of danger and the uncertainty of the ultimate fate which awaits them, produces irregularity in the action of the heart and respiration, and relaxation of the sphincter muscles—in other words, a mild degree of shock. If wounded just at this time the shock is

more pronounced that if it occurred during the heat of an engagement. When engaged fully in action, this feeling of dread soon passes away and is forgotten in the excitement of the battle, and now the severest wounds are received, and shock is deferred for hours, and not experienced until the stage of excitement has passed away. Then it is that the mind takes cognizance of the wound and its gravity, and shock rapidly supervenes. The higher nervous centres being preoccupied, have contributed nothing to its production, and at the same time have held in abeyance and controlled the various reflexes which the irritation of the nerves of sensation at the point of injury have started into activity. Hence, shock is deferred. If it were possible to maintain for an indefinite period the mind in this state of control of the reflexes, we should be able to obviate the supervention of shock arising from physical injury.

In the severe injuries sometimes received by men while in a state of profound intoxication from alcohol, the shock is not marked, especially if they are made warm and comfortable, and stimulants are not suddenly withdrawn. The effect of alcohol is similar to that of ether and chloroform; the nervous centres, from the highest to the lowest, succumb to its influence in the same order of procession. During the state of profound intoxication, consciousness, sensation, and all the reflexes are temporarily held in abeyance, and it is impossible for a nervous impulse originating in any part of the brain, or the irritation of an afferent nerve, to be transmitted to the centres which are instrumental in the production of shock.

In injuries arising from accident, gunshot, and which occur in the ordinary pursuits of life, it is not in our power to prevent the shock which subsequently follows, but much may be done to relieve it and thereby save life.

The chief indication in the treatment is to relieve promptly the embarrassment to the circulation, and, as this is owing mainly to undue stimulation of the vagi, remedies, such as atropine and strychnine, which have a sedative or paralyzing

effect on these nerves have the happiest effect. These drugs ought to be administered together hypodermatically in doses of one-hundredth to one-fiftieth grain, and at the same time external heat applied to maintain the warmth of the body. If the patient can swallow, whiskey or brandy should be given, hot, and but sparingly diluted, in order, as Lauder Brunton says, "to obtain its stimulating reflex action on the heart." The latter can frequently be stimulated into action by firm but intermittent pressure, by the end of the fingers, of the ribs immediately over it. The abdomen should be bandaged with a flannel roller, as it maintains warmth and by its pressure relieves somewhat the plethora of its blood-vessels.

There is much discrepancy of opinion in regard to the administration of morphine hypodermatically just before the anæsthetic is given in cases requiring a surgical operation, and when deferred this late it is of questionable propriety. But it is of invaluable service not only in mitigating, but in preventing in a large measure shock when administered in a proper manner and interval before the contemplated operation. The patient should be prepared and all information of the time of the operation should be withheld from him, and without arousing his suspicions, a hypodermatic injection of morphine and atropine given in a sufficient dose to produce sound sleep; and while in this state, the anæsthetic should be cautiously administered so as to bring him completely under its influence without awaking him.

All the phenomena by which shock is manifested are the result of reflex action through the medulla and are caused by impulses originating in the great nervous centres of the brain. Any injury or irritation of the peripheral extremity of an afferent nerve must be propagated to these centres before it can reach the great centre of reflex action. To prevent shock, therefore, we must endeavor to avoid exciting any thought or emotion which may start these impulses from the great centres to the medulla, and give rise to those disturbances which constitute its essence. When a patient undergoes

preparation for a surgical operation, and is told, even days before, of the time and place, and is borne into a modern operating-room with all its paraphernalia of trays and knives, sponges and appliances for rendering a wound aseptic, and sees the preparations being made by surgeon, assistants and nurses, he must have a stout heart and be more than human to view what is taking place around him with stolid indifference and without fear and apprehension as to the final result. Before he receives the usual hypodermatic of morphine, or the anæsthetic is administered, he is like the soldier approaching the battlefield—he is already suffering from a more or less mild form of shock. Add to this the stimulating effect on the vagal centre of the primary action of the morphine, and if perchance the first cut of the surgeon's knife is made before he is fully under the influence of the anæsthetic, we have an additional inhibitory stimulus which may bring the heart to a standstill and death ensue. Whatever thought or emotion dominates the mind at the moment loss of consciousness occurs from sudden seizure of disease, a traumatism of the brain, or the effect of an anæsthetic, the same thought or emotion on the return of consciousness will still exert a controlling influence, and may give rise to a motor response, or diminish or increase the blood-supply to some particular vascular area. If a thought or emotion powerful enough to excite reflex action occupies the mind as it passes under the influence of the anæsthetic, it will reappear in undiminished vigor as soon as intelligence resumes its sway.

On one occasion, while conversing with a gentleman of more than ordinary intelligence, a president of a railroad, on a subject in which he was deeply interested, he was suddenly seized with an epileptic convulsion while speaking in the middle of a sentence. The fit was rather a severe one and was followed by coma which continued for twenty-four hours, and, happening to be sitting by the bed when consciousness returned, he finished the sentence which was interrupted by the fit. Some time ago I was called to see a man in a state of profound coma from a compound, depressed fracture of the right

frontal bone near the median line, the wound having been inflicted during a quarrel by the small end of a monkey-wrench in the hands of his antagonist. As soon as I trephined and removed a small button of bone which had penetrated the dura, and, further, had raised the depressed fracture, consciousness returned and the man resumed the quarrel.

Shock is sometimes not very pronounced from the moment of the injury, yet persists for hours and days before full reaction finally takes place. While the artery at the wrist seems to be full enough it is weak and compressible, and the patient is in a state of somnolence—sleeping most of the time. The impression of the injury on the nervous system is more profound than the early symptoms would seem to indicate, until its recognition is forced upon us by some marked disturbance of nutrition. Apart from the destruction of tissues inflicted by the injury, shock through disturbance of the whole nervous system effects very serious changes in the nutrition of the body, and will cause diseased action in any part from slight irritation, and the skin is especially vulnerable in this respect. The pale and livid hue of the surface of the body is evidence of its innutrition. The blood which nourishes it, being propelled by a weak and enfeebled heart, flows with diminished velocity through arteries and arterioles of diminished calibre, and cannot convey the requisite amount of nourishment to the various tissues of the body. Hodgkin, as quoted by Erichsen many years ago, in 1857, suggested that the condition was due to a chemical poison formed in the wound from innutrition of the injured tissues, and being absorbed into the circulation poisoned the system—in other words, was caused by a ptomaine. The real cause is owing probably to the presence of leucomaines which have been produced in excessive amount from general innutrition of all the tissues of the body from an inadequate supply of nourishment. The impaired vitality and lessened power of resistance of the skin to external irritants is marked, and sometimes the death of a skin flap can be attributed solely to this cause. Hence, when a patient has received injuries which

render an amputation imperative, and the shock has been persistent, the flap, including some muscle, should be the preferred method, as the small amount of muscle which it includes secures greater vascular supply.

In severe shock the presence of albumin in the urine can usually be detected, and in the persistent form of it, becomes a serious complication, as it retards the healing of any wound which may be present, or may cause the system to succumb to septic influences. Albumin is not the direct result of a wound or injury to the tissues, but arises from the effect of the profound disturbance of the nervous system which constitutes shock on the organs chiefly concerned in eliminating effete matter, from the body. It was a happy circumstance that the man who was removed from the pit in Cheraw in a helpless condition from shock, was examined the previous evening by my friend Dr. Kollock, for life insurance, and the urine tested for albumin and none was found. A few hours after the accident the urine was again tested and albumin was so abundant as almost to solidify the contents of the tube.

DISCUSSION.

Dr. A. M. Cartledge, of Louisville.—I rise to commend the excellent paper of Dr. Evans. In this day of business surgery, so to speak, when we go along and do so many more operations than we formerly did, the mental element in shock is an important one, and we are inclined to forget it. We forget to encourage our patient. The paper is a timely one, and warns us to be careful.

Dr. Edwin Ricketts, of Cincinnati.—There is one thing certain—a patient going to be operated on should not see any of the paraphernalia of the surgeon. Too little attention is paid to this. I never allow my patients to see any of the preliminary preparations that are going on. The anæsthetizer does his work, the operation is performed, and the patient does not see the instruments that are on the table. I am in full accord with the essayist and the gentleman just on the floor (Dr. Cartledge), that not enough attention is given to the moral aspect of the subject,

and patients should not be subjected to such an ordeal as they are sometimes. They should not witness the trays or any of the instruments that are to be used in the operation.

Dr. Cornelius Kollock, of Cheraw, S. C.—I was very glad to hear Dr. Ricketts make the remarks he did, and I agree with him, that we should keep all the instruments out of the sight of the patient, and allow no one to go into the room except the man who administers the chloroform. The case Dr. Evans mentioned happened near my town. An iron bridge was being built across the Pee Dee River. A pit was dug forty feet deep, and an immense block of granite, weighing 2300 pounds, was being lowered into it. At the bottom was a man who was to direct it into the proper position. In lowering it the cable slipped. The man backed up in a corner, and the stone brushed him on the cheek as it fell; it hardly broke the cuticle. The men spoke to him, but he could not answer them. When they got him out he looked like a dead man. I did nothing to him, except injecting a little morphine under the skin. That revived him a little. The night before I examined the man for life insurance, and never examined a healthier man in my life. The morning after the accident the urine was heavily charged with albumin. The presence of albumin in the urine in this case was caused by the shock from the accident the day before. The man was not injured at all.

Dr. R. M. Cunningham, of Birmingham, Ala.—I expected to hear Dr. Evans speak of hot saline injections into the rectum in the treatment of shock. I endeavor to inject the rectum, every half-hour or so, with saline solutions. Patients have been benefited by injecting saline solutions into the femoral artery with a fine aspirator-needle, according to Dr. Dawbarn, of New York.

Dr. Gaston.—Why the artery in preference to a vein?

Dr. Cunningham.—I do not know why. One word in regard to the preparation of the patient for operation. Surgeons who have private hospitals and who operate in public hospitals, can withhold from the eyes of patients a great many things which influence shock; but what are we to do where we have several negroes, for instance, on a railroad living in a cabin ten feet square? Are we to make all our preparations on the outside? I do not believe the mental impression is as great as some of the speakers would have us believe.

Dr. Geo. A. Baxter, of Chattanooga.—In regard to mental impressions, I want to relate a case which came under my observation when a student of Dr. James R. Wood, of New York City. A gentleman who lived in England—and Sir Henry Thompson was operating at that time for stone—came to America to be operated on by Dr. Wood. On his applying for operation there was nothing whatever in his manner to indicate abnormal mental disturbance. He was operated on and died promptly. There was not a single indication of trouble at the time of the operation. The post-mortem disclosed nothing wrong. In his effects was found a statement that he came to America knowing he would die. He was unable to endure the torture incident to the existence of the stone, and his family opposed the operation so strenuously at home that he came to America and gave instructions how to forward his body back. The man died of mental shock.

Dr. Gaston.—Did you use an anæsthetic?

Dr. Baxter.—Yes, of course. He did not die from that. Shock is of many characters. Its manifold appearances to me are as different as the skies. To me the most dangerous kind of shock is where the patient has absolutely no control of himself, and is perfectly indifferent to his surroundings or results; and in the majority of cases of shock, instead of the mental impressions being blunted, they are more acutely sensible, in a general way, of what is going on than they were before the reception of the shock. A peculiarly dangerous condition to me is in those cases where they hear a determination to cut off a limb with stolid composure; worse, perhaps, than that, they are totally and entirely indifferent to any results that may seem to follow from it. Shock varies not alone with the amount of tissue injured, but it varies with the character of force that has produced it. That is why I account for the increased shock in railroad injuries. When two bumpers come together, the cars are seldom on a straight line, and the bumpers are more or less at an angle. There is a rolling, continued, exerted force—a more complete nervous destruction than there would be from the force exerted at once. There is a twisting motion that produces greater destruction of the nerve filaments and paralyzes the peripheral ganglia, producing many manifestations of shock, especially that of the circulation. It is this twisting motion that produces a

deeper and wider impress and destruction of the nerves. I sometimes think there is more or less injury of the tissues further removed from the immediate point of injury. It is in this way, and this way chiefly, that I believe an increase in shock in railroad injuries can be accounted for. On the other hand, the majority of railroad injuries are produced by an increased crushing force; that the machinery is of a large, ponderous character; that the man is in a state, usually, of entire unexpectation, when suddenly an immense force comes against him in some way. There is a mental impression produced by it, and you cannot account for the impress unless it be of a reflex character. It is a distinct shock, positively reflex in character, and independent of any impress that may be made.

I agree with the Doctor in regard to injections. I received my impression not from the source he did, but I remember during the excitement of cholera in this country this year, that in some of the French papers there were statements made to the effect that during the collapsed condition of cholera the injection of hot saline solutions might prove beneficial. The idea struck me that if it was good in the collapsed condition of cholera, it would be a good thing to equalize the circulation and restore warmth to the system. I tried it, and found it beneficial in several cases.

Another point I would mention in connection with the subject of injections is this: There is nothing better to precede any operation where there is a great loss of blood than the injection of hot saline solutions, not only for shock, but to counterbalance the loss of blood. I had a case recently, of a large osteo-sarcoma of the thigh, and would have lost the patient on the table had it not been for the previous provision of a colleague, who was present, in having a hot saline solution ready when the pulse was gone at the wrist. The patient quickly recovered; the pulse filling under it very promptly; the rectum seemed to absorb it, in this condition of the system, almost as a sponge absorbs water.

Dr. W. O. Roberts, of Louisville.—I do not see how saline solutions can relieve shock, except it be from the loss of blood.

In connection with what Dr. Baxter had to say in reference to railroad injuries, it strikes me that where the injury is not severe, the mental shock cannot be great, except from the narrow escape that the man has met in receiving the injury. Not a

great while ago, in attempting to board a train in motion, I caught hold of the railing of the wrong car and swung between the cars. When I recovered myself the mental shock was considerable. That would be the condition of things in cases where the injuries are slight, as injuries of the hand.

Dr. Cartledge.—When I noticed the title of this paper on the programme I hoped that the author or some one in the discussion would forever divorce the subject of shock from acute anæmia, or the loss of blood. I am sorry they have not done so.

With a clearly defined idea of the pathology of the two affections, I fail to see how the injection of saline solutions can influence shock. Certainly it would be admirable practice in acute anæmia. While shock and acute anæmia have many symptoms in common, the treatment should be in many respects diametrically opposite.

Dr. Evans.—In reply to Dr. Cunningham, I approve of keeping the patient warm. What is the condition of shock? We have a firm contraction of all the smaller arteries and arterioles of the surface of the body, and at the same time we have a bleeding, as it were, internally, as the whole of the bloodvessels of the abdominal viscera are engorged. Heat applied externally dilates these vessels, attracts blood from the internal organs to the surface of the body, stimulates the action of the heart, and thus contributes to the relief of shock. When a large quantity of hot water is thrown up the lower bowel it mechanically forces the blood from the abdominal viscera, and if retained for a few minutes, a further expulsion of blood takes place from the firm contraction of the bloodvessels as the result of the prolonged action of heat on them. I do not think when we are called to see an individual suffering from shock, that any remedy is equal to atropine and strychnine. It paralyzes the vagi, and the heart beats with more force, and reaction more speedily takes place than from the use of any other remedy.

The point brought out in my paper in regard to giving morphine did not seem to impress many of the members. If you give it at all, give it before the anæsthetic is given, and it will prevent shock just as it does when a man is profoundly intoxicated.

A MANIPULATIVE MISTAKE AND ITS CONSEQUENCES.

By George Ross, M.D.,
Richmond, Va.

It is with no expectation of enlightening this body of distinguished medical men that I briefly present a case illustrative of a "Manipulative Mistake and its Consequences," but rather with the purpose of impressing the importance of vigilance and care in the simplest and most common surgical procedure. It may not be unseemly that I apologize for consuming even a few moments of the time of this Association in the bare recital of an accident primarily so simple, but its novelty and serious results have seemed to me sufficient justification for placing it on record as a *possibility* hardly likely to be duplicated during the life of any of those to whom I address myself.

It was ten years ago that, as consultant, I came to know Mrs. W., of Richmond, Va., my aid being sought by her physician in his efforts to relieve her of a long-standing "dysmenorrhœa." She was a perfectly formed woman, had been married two years, had never conceived, and, eliminating this trouble and an occasional attack of "vesical irritation," was perfectly healthy. Her physician had already administered the usual constitutional remedies prescribed in such conditions: nothing bettering her, and having inherited her from another physician at whose hands she had received local treatment, he was specially solicitous for her recovery. A specular examination at once revealed "cervical flexion" with "stenosis" demanding and receiving "divulsion," followed

by hot douches and glycerin tampons. Relief of the "dysmenorrhœa" was the immediate consequence and "conception" followed within the year. I was retained for the "accouchement" and delivered her under chloroform, after a painful and protracted labor, of a male infant. Then began the pronounced attack of "vesical irritation" of which she had had mutterings during her gestation and previously. For its relief she generally found solace in some domestic remedy. Occasionally my aid was invoked, when a few doses of some "alkaline diuretic," supplemented by rest in the horizontal posture, would rout the enemy.

Two years passed and another boy baby came, this time without difficulty or complication. She had a normal "getting up," nursed her baby, and was apparently well. But the old enemy was still in ambush and declared itself a power for bringing distress on its victims at varying intervals. Frequent examinations of urine showed the kidneys to be healthy. The patient's appetite was always good, digestion perfect, never a fever. Why the vesical pain, often vesical agony? Could it be stone? The sound said, No! Could it be an "irritable caruncle" or "fissure" at the "vesical neck"? Ocular inspection and urethral dilatation said, No! "Acute cystitis was a suspicion, but there was no fever and no "hypogastric tenderness." She was deluged with the full gamut of diuretics and Lithia water and sweet milk and hot vaginal douches and enforced rest, and relief would come, but temporary relief only.

Pregnancy again became a factor to mask the symptoms and exaggerate the patient's discomfort. A third boy saw the light without unusual incident. Convalescence was prompt and satisfactory, but was hardly established before a vesical explosion took place; this time more terrible than ever, and palliated only by frequent administration of morphine. Day and night it went on, this unremitting, agonizing "tenesmus." Drop by drop the urine flowed, at last only to find exit through a soft catheter, which she was taught to use and through which antiseptic and sedative douches were applied

to the viscus. Then it became impossible for her to insert the catheter; some immovable obstruction was present, and I was obliged to go to her rescue. I had no difficulty in sliding the instrument along the floor of the urethra and bringing away a full flow of alkaline urine. Why her embarrassment, and why the tormenting, agonizing straining, so unceasing and so violent as to force the "uterine os" to the very verge of the "fourchette." Days dragged despairingly along, and nights brought no surcease of sorrow and suffering. Could a human being endure such unremitting torture and live? Courage, brave Christian woman! The day dawn was breaking. Make yet one other mighty effort, and a thankful Eureka! shall be my greeting at the early morning visit. Exultantly she pointed me to a vessel in which had been placed the "mass" which for seven years she had carried in her bladder and made her life so miserable. I wondered what its constituent elements could be, and inspected it, to find—what? A pledget of absorbent cotton, once saturated with iodine, in shape a truncated cone, and thinly incrusted with phosphate of lime. How did it get there? you ask. It was impossible for her to have introduced it, even if she had desired to do so. She believed it to have been the work of her first physician when attempting to apply his intra-uterine dressing, mistaking the "urethra" for the "cervical canal." He was an *unmarried* man, she said, and his successor being a *married* man would surely have known the difference between the urethra and vagina. Its expulsion was followed by a slight hemorrhage, the result of a mere scratch of the urethral mucous membrane by the phosphate shell, and its "consequences" were most grievous. Two days of rest and peace and sweet refreshing sleep were passed, and then a chill, a fearful chill, and fever and sweat, to be followed by another and another during that and succeeding days. Purulent absorption had taken place at the site of that scratch, and now "pyæmia" was monarch. Scylla was escaped; Charybdis was like to wreck our frail and long-tossed human bark. Two weeks of anxious watching and waiting and constant nursing were

passed, and my patient had responded to the abundant food and stimulants, and quinine and sulpho-carbolate of soda, and antiseptic douches with which she was saturated and kept clean. She was well again—as well as one might reasonably hope to be after so severe and protracted an illness. But, alas! for the presence of that long-resident, fleecy foreigner in the bladder, a persistent "chronic cystitis" was engendered, and notwithstanding the faithful and continuous exhibition of extracts of pichi, corn-silk, pareira, buchu, copaiva, santal oil, iron, and drainage, *et id genus omne*, she still suffers from it, and the end seems to be not yet.

OPERATION FOR HARE-LIP WITHOUT THE PIN.

By William Perrin Nicolson, M.D.,
Atlanta, Ga.

The only excuse for offering the brief paper on this subject is the fact that practically all of the text-books on surgery, both old or new, insist upon the use of the hare-lip pin in all operations for this trouble, whereas my experience has been such as to convince me that it is not only unnecessary but harmful in most cases.

We all know that in former times union without suppuration was rare, and it was necessary to provide some method of holding the tissues in contact until the slower process could be accomplished, and the pin was perhaps the most suitable means for accomplishing this end.

With the methods of to-day it is feasible to secure primary union in all of these cases, and, as a matter of fact, a few days only are necessary for the formation of a strong union. Thus, there is little necessity in most cases for any more support than is given by ordinary sutures. It has been my practice for a number of years to close all operations, as well as accidental wounds, involving the entire thickness of the lip with a simple suture, and it occurred to me that those of hare-lip could be just as effectively handled in this manner. The results of my practice have demonstrated conclusively that the simple suture is all that could be desired.

In investigating the question of the most available suture I have experimented with a number of kinds, and the simple catgut has seemed most satisfactory. In my opinion it is well,

in children, to use an absorbable suture whenever practicable in order to avoid the necessity for removing them—something that, as a rule, with non-absorbable sutures, can only be accomplished under an anæsthetic. For this reason the silk, silkworm-gut, and chromicized catgut have seemed to me unsatisfactory, though it is undoubtedly true that the silk-worm-gut is, in many respects, the ideal suture for plastic operations. On account of its freedom from irritating effects it has been especially recommended by Mr. Treves for uniting the wound of hare-lip. The only objection to its use is the fact that its removal amounts to a small operation in the case of children.

The objection that is generally raised to the application of the simple suture is the inability to sustain the necessary tension that is regarded as a part of the operation in hare-lip cases. The only reply to this is that in almost all cases the most essential step to secure a good result is the entire relaxation of the wound edges by free separation of the lip so as to get rid of all tension. In very wide fissures, where some tension necessarily remains, I know of no means so efficacious in overcoming it as a relaxation suture of silkworm-gut, carried beyond the wound borders, and looped over pledgets of iodoform gauze. I have seen such a suture remain *in situ* for two weeks, in a case where there was a disappearance of the union by ulceration, and in the end the result was very good. It is a noteworthy fact that there had been two previous failures in this case in other hands where the pin was used, and had to be removed before union could be accomplished. I do not doubt that failure would have been the necessary result had I used the pin.

The after-treatment of the wound consists in applying over the wound, from cheek to cheek, a flexible rubber plaster, tongued so as to permit of traction upon both pieces in drawing them together. In almost every case this will overcome the traction upon the sutures, and it can remain in position until union is firm. In adult cases I would not hesitate to close the wound under an iodoform scab, as I have frequently

done in accidental wounds of the lip. Where the plaster has to be applied, I know of none so suitable as the rubber plaster spread on silk, made by Johnson & Johnson.

The superiority of this treatment of the wound over the use of the pin I claim is that it is more in keeping with the surgery of this day; that we escape the necessary ulceration and consequent marking by the pin; and that it practically completes the operation at one sitting. I am firmly convinced that the day is not far off when the use of the hare-lip pin will live in surgery only as a memory.

DISCUSSION.

Dr. A. M. Cartledge, of Louisville.—I indorse what Dr. Nicolson has said in regard to plastic surgery about the lips; we should dispense with the pin, for it has nothing to support it but tradition. It has been my practice to use an ordinary suture. For this purpose I think silk is preferable. Where you want more relaxation introduce a relaxation suture. These cases do most beautifully under a scab, and I would cover it with iodoform collodion.

Dr. W. C. Dugan, of Louisville.—I used a hare-lip pin for a while, but recently I have been using a silkworm-gut suture. I think it is better than a pin, and superior to any other suture material. I remove the sutures usually at the end of seven days. The silkworm-gut, causing no irritation, can be left in a long time. If the patient is a child and disposed to cry, I give it a little chloroform and remove the sutures.

Dr. Cornelius Kollock, of Cheraw, S. C.—I did a hare-lip operation on a girl twelve years of age; she was a pretty girl, but badly disfigured. Her mother was a fastidious woman, and requested me not to use any stitches about her mouth. She said, "Can't you close it without stitches?" I replied I could not promise that it would hold. I applied an adhesive strip, with a pack of collodion over it, and it held beautifully.

Dr. W. O. Roberts, of Louisville.—In 1886, at Schede's clinic, I saw him do many hare-lip operations. He used no pins at all, but silkworm-gut and catgut. A good deal depends upon the way in which the operation is done. In many cases one side of the lip is very much smaller than the other, and in

doing these operations he pares the lip off at a right angle some distance below, then he saves the tissue of the lip in bringing it around to make up for the loss of tissue of the other lip. In that way the tension is much less. The silkworm sutures are carried nearly through the lip, so as to bring the deeper structures together as well as the skin. Practically it holds as a hare-lip pin. The after-dressing, as Dr. Cartledge says, is best done with collodion. I always use, in addition to the collodion, a saturated solution of iodoform in collodion. I have never seen any good derived from the application of adhesive plasters.

Dr. William T. Briggs, of Nashville.—I have been in the habit of using the ordinary interrupted suture for a number of years in hare-lip operations, sometimes silk, and sometimes catgut. In witnessing an operation done yesterday by Dr. Marcy, for radical cure of hernia, I was impressed with his method of buried sutures, and thought it would be the best suture to apply in the treatment of hare-lip. It brings the parts together, yet it is under the skin. Over that is applied collodion and iodoform. It struck me that a stitch like this would be admirable for the cure of hare-lip without the formation of stitch-hole scars. It does not matter, however, what method of suturing, or what suture material is employed, the parts will unite if properly pared and accurately approximated.

Dr. Nicolson.—I will simply say that the discussion bears out what I predicted when I started, that a great many operators are following the method of using sutures instead of pins, but yet. I have taken the pains to look over the text-books that are presumed to teach us these things. Even in the latest text-book in American surgery I find the old hare-lip pins recommended, and I have not found anything but Treves's work that coincides with us. As Dr. Cartledge has said, the hare-lip pin has nothing but tradition to support it.

In regard to a collodion dressing, I believe our operations will be more satisfactory with it. I have found a strong tendency to the wound becoming infected, the child crying, and the secretion running down over the wound. As far as the support of adhesive plaster is concerned, I used two strips right over the lip, more for fear the child may scratch it than anything else. I saw one case where the pin had been removed. On the fifth day the child made a scratch and tore it from the entire nose down. I subsequently made an operation and secured a perfectly good result.

CHOLECYSTOTOMY.

By Edwin Ricketts, M.D.,
Cincinnati.

Cholecystotomy is an operation for a diseased condition. I wish to report the following case:

Mrs. S., a German, aged thirty-four years, married for five years, no children; consulted me in June, 1892. She said that her trouble made its appearance about eighteen months previous, when she suffered from jaundice, tenderness over the region of the liver, loss of flesh, and had clay-colored stools; she then weighed one hundred and eighty pounds; the attacks of pain in the region of the liver were not severe in character, nor was jaundice marked; however, she did have attacks of vomiting.

Her physician made a diagnosis of gall-stones, put her on a restricted diet, and gave her phosphate of soda internally.

There was, for awhile, temporary relief, but never in the whole eighteen months did the patient cease to lose flesh, and when I saw her for the first time she had lost thirty-five pounds in weight. Upon examination there was tenderness over the liver, but no marked distention; upon deep pressure over the region of the gall-bladder through the very thick abdominal wall, no distention of that organ could be felt, but pain was more severe at this point than over the liver proper; I advised an exploratory incision to be made over the gall-bladder as an aid to diagnosis; the same was done, and on exposing the gall-bladder it was found to be very tightly distended and filled with fluid; no nodules of the anterior or posterior surface of the liver could be detected.

Upon placing my index-finger against the duct it could be easily mapped out, owing to its distended condition, and a stone was felt down in the common duct.

Massage was resorted to, hoping that in this way we might be able to force the contents of the gall-bladder through into the duodenum, as I had done in two previous cases. Here, however, I was unsuccessful, wholly so, in fact; and then I resorted to packing the surrounding field of operation with flat sponges wrung out of hot water; after this was done as well and carefully as possible I proceeded to puncture the gall-bladder with a small scalpel; immediately a considerable quantity of clear fluid spurted out, in which there was no trace of bile apparently; the force of the fluid demonstrated the great distention present.

After thoroughly sponging away the fluid I introduced one end of my ten-inch silver coin olive-pointed probe down into the common duct with little difficulty, and succeeded in striking the stone at once; I now carried my left index-finger into the abdominal cavity, placed the palmar surface of it against the common duct at the point through which the stone could be felt; by this I was able to employ bi-manual pressure, and could thus help matters very materially.

After passing the probe at some distance beyond the stone, keeping alongside of the duct wall, I withdrew it (the stone, weighing twenty-three grains) with a pair of Tait alligator forceps, which are ten inches in length; I hoped thus to be able to dislodge the stone, and was finally successful.

Fearing that I might have a stone to contend with in the hepatic duct, and that it might fall into the common duct after the completion of the operation, I passed the probe, already curved, as far into the duct as possible, and satisfied myself that the hepatic duct was free; after this the straightened probe was passed into the common duct to the duodenum, and I was then satisfied that the biliary ducts were all free.

I now stitched the edges of the incised gall-bladder to the edges of the incised peritoneum; transverse stitches of silkworm-gut were put in through all parts, and a long glass drainage-tube of the Price pattern was inserted, the perforated end of which was pushed low down into the common duct, and the wound was stitched up; through this glass tube warm water was forced by means of a syringe; this was done while the patient was turned on her side. Some débris was washed out at the end of the second day; the bowels were moved by means of saline cathartics. Bile began to make its appearance on the

third day, and the tube was removed; the patient made a satisfactory recovery, with the exception of a stitch abscess of the thick abdominal wall; this was treated with injections of peroxide of hydrogen at frequent intervals. The patient has regained her former health and flesh, and attends to her household duties.

I reported this case for the reason that there was no history of marked jaundice or of severe attacks of pain (hepatic colic), so usual in these cases. The main symptoms that prompted me to resort to an exploratory incision as an aid to diagnosis were:

1. The presence of clay-colored stools.
2. Loss of considerable flesh.
3. Tenderness over the liver, most marked over the region of the gall-bladder.

As I said in the beginning, cholecystotomy is an operation for a diseased condition, and I fully agree with Dr. Hal C. Wyman, who, in a recent paper before the Mississippi Valley Medical Association, took the ground that this operation promises much in the relief of certain engorged conditions of the liver by affording free drainage. We cannot compare the experimental work upon the bile-ducts of the lower animals with the operations upon the diseased gall-bladder in the human being.

The advantages of cholecystotomy over cholecystectomy, or any operations upon the gall-bladder, are to me apparent; I mean over those operations where the gall-bladder is lifted up and dropped back into the abdominal cavity; much debris can be drained from the bladder after the finished operation by syringing through the drainage-tube, and even some stones can be removed through this tube that may have been overlooked and finally have dropped down from the hepatic duct. Or, if the stone is too large for this, the tube can be withdrawn carefully, and then the stone be removed whole or after it has been crushed with forceps. As to the positive relationship of gall-stones to cancer of the liver, beginning in the bile-ducts,

I am quite satisfied that such is true; in two cases which I opened the abdomen for obstructed gall-duct I found in one case a large stone, in the other five; cancer was present in both of these. As cancer of the liver usually runs its course within two years, I am satisfied that the presence of the stones antedated the cancerous conditions; I do not mean to say that all cases of gall-stones must necessarily have cancer to follow; but undoubtedly in many cases, especially in those above fifty years of age, the presence of gall-stones in the common duct undoubtedly plays an important rôle in the excitation of cancer.

DISCUSSION.

Dr. Joseph Eastman, of Indianapolis.—I have been interested in the paper of Dr. Ricketts. I have opened the abdominal cavity for gall-stones nineteen times; I have sixteen of the specimens with me (here the speaker showed specimens of sixteen cases). The other stones were impacted, and so crushed in their removal that they were hardly worth preserving.

There are one or two points in connection with the surgery of the gall-bladder that I feel constrained to call attention to. I differ with much of the teaching of to-day. I see it constantly reported that surgeons find stones in the common duct. I taught anatomy for seven years, and, I might say, in my anatomical demonstrations I believe I learned to determine where the common, cystic or hepatic duct was, fairly well. I have opened the abdomen 400 times, and why it is I have never found gall-stones in the common duct I cannot say. I have found patients with clay-colored stools, and after removing stones from cystic duct the stools contained gall. I have found that compression of the common duct was due to distention by the pressure from accumulations of gall-stones in the gall-bladder and in the cystic duct. I am not saying that Dr. Ricketts was mistaken in his case. I am ready to concede that in his case there was a stone in the common duct; but I am at a loss to know why he and others should find something that I have been unable to find.

Within the last few years the operation of cholecystotomy has been made so often that it is quite probable, in my mind, that

much of the literature we have on the subject had better be rewritten, as it has been written from books that were gotten up by the theorist of the closet, or from the book-worm of the library. In other departments of abdominal surgery text-books will have to be rewritten. With reference to detaching stones from the cystic or common duct, it has been suggested, in an article in the *Virginia Medical Monthly*, to cut open the duct to remove the stone. With what little experience I have had, I should say that that is not good practice. I believe it is better to introduce a drainage-tube in the gall-bladder, with the hope of the stone becoming disengaged, and either absorbed or discharged in some way, than to open the duct. I doubt the possibility of a suture that would close the duct nicely, without impairing its function, so that there would be a discharge of gall in the bottom of the cavity. When gall has been in the gall-sac for a considerable length of time, and there has been more or less peritoneal inflammation or cholecystitis, as we find in many of these cases, I doubt whether that gall would be as harmless in the peritoneal cavity as pure gall. I have on two occasions (both patients recovering) crushed a stone in the cystic duct as large as the end of my finger. I have been able to crush with forceps by slipping a piece of rubber tube as large as my finger on the point of forceps. I have on several occasions found fragments of stone that were washed out of the tube in gall-bladder.

When the patient strains, vomits, or retches, there is force enough to send it out through the drainage-tube. A drainage tube inserted into the gall-bladder will relieve a great many chronic conditions of the liver. I have found gall-stones in connection with cancerous diseases of the liver, and have been teaching that obstructed gall-ducts stand in a causative relation to that disease.

Dr. Arch. Dixon, of Henderson, Ky.—I have seen cases of impaction of the common duct with gall-stones. A number of years ago a man fell from an elevator in our town and ruptured the gall-bladder. I did a cholecystectomy on him, and the case was reported in the *Annals of Surgery*. The man did well for a time, but subsequently died of a cholic diarrhœa. Autopsy revealed an impacted gall-stone in the common duct, which precluded the possibility of the passage of bile into the intestines.

Dr. J. McFadden Gaston, of Atlanta.—I have not had an

extensive experience in connection with operations of the gall-bladder for a diseased condition, but I have investigated the subject very thoroughly, and I will endeavor to enlighten my brethren on obstructions of the gall-bladder by stating that the common duct is not only obstructed in some cases by gall-stones, but that gall-stones have been extracted a number of times from the common duct by incision. Two of these operations have been done in this country, one by Dr. Marcy, and the other by Dr. Vander Veer. There is no question about the matter of gall-stones forming in the common duct and obstructing it.

In regard to catheterization of these ducts from the gall-bladder, through the cystic duct and into the common duct, I will say that if Dr. Ricketts has succeeded in doing what he describes, he has done what has never been done before. If he has passed a forceps from the gall-bladder through the cystic duct into the common duct, and extracted a stone from the lower part of that duct, he is entitled to priority in that line of operation. It has not been done in the history of gall-bladder surgery prior to this time. Catheterization of these ducts is regarded as one of the most difficult things in connection with gall-bladder surgery. The passage of a curved sound from the gall-bladder through the cystic duct into the common duct, which we know is almost an acute angle, is attended with a great deal of difficulty. Dr. Ricketts' statement, that he has succeeded in passing a glass drainage-tube from the gall-bladder through the cystic duct into the common duct, is remarkable. I do not intend to throw any discredit upon what he has said, but there must have been some remarkable anatomical or pathological condition for him to have been able to accomplish this result. The practicability of catheterization of the common duct through the cystic duct is one of the problems to be solved.

The operation of cholecystotomy is one that has been very frequently performed. We are aware that this is Mr. Tait's famous mode of relieving all obstructions connected with the gall-bladder. He claims to have secured very remarkable results, and no doubt has, where the common duct was obstructed, by removal of the gall-stones. Dr. Ricketts tells me that the gall-stone in his case was removed without crushing. Obstruction in the common duct is the great difficulty which Mr. Tait's operation cannot relieve. The simple attachment of the gall-bladder to the

external parietes is not likely to relieve a stone where you have agglutination of the walls of the common duct. I had the misfortune to operate on a case of this kind where the agglutination of the common duct was such that no relief was given. Bile poured out freely through the opening in the external wall. Another case of a similar character, under my observation, had the same termination. The obstruction continued, and the only relief given in these cases is to bring about direct communication of the gall-bladder with the intestinal canal, either with the duodenum or small intestine.

Dr. W. E. B. Davis, of Birmingham, Ala.—I regard the case reported by Dr. Ricketts as an extraordinary one. I cannot see how it is possible to introduce a tube as Dr. Ricketts has done, unless there are very marked pathological changes. The obstruction must have existed for a long time in order to produce such an extreme dilatation of the duct.

As to the treatment of these cases, I think the crushing of these stones through the duct with a forceps is a risky procedure. In doing this you cause such injury to the duct that you are liable to have sloughing afterward, and escape of the contents of the duct into the abdominal cavity. The method of puncturing with needles is also a risky procedure. Unless the stone can be dislodged either into the intestine or back into the gall-bladder, to my mind it is much wiser to incise the duct and remove the stone. Of course, all operations for stones in the common or hepatic duct are serious. The patient is suffering from cholæmia, and is in an unfavorable condition for an operation. These operations do not compare with those for stone in the gall-bladder. They are operations of much greater mortality. Returning to the operation, it seems to me that the better plan would be to incise the duct, and clean it out thoroughly, and if you cannot stitch it satisfactorily, to pack around it with iodoform gauze.

I have conducted some experiments to test the value of gauze in draining bile from the gall-bladder and ducts. I have removed the gall-bladder without tying the duct, by packing with iodoform gauze, which was removed on the third day; the animal got well without serious trouble. I have also incised the gall-bladder, packed gauze around the opening, no stitches being used, and the animal did well. Similar experiments have been conducted by others, and from them we learn that if an animal

can get well by packing with gauze, without any stitches, it is safe to rely upon the gauze in opening the duct and removing the obstruction. A glass tube might be used with the gauze.

Dr. Eastman.—In one case I crushed the stone with my fingers; in another, with forceps, and recovery was everything that could be asked for.

Dr. Ricketts.—I said in the beginning of my remarks that cholecystotomy was an operation for a diseased condition, and experiments upon dogs that have healthy gall-bladders, healthy common ducts, I do not think should be taken into consideration with an operation of this kind upon the human being. In the removal of small cystic ovaries, are they to be compared with an ovarian tumor weighing fifteen or twenty pounds? An ovarian tumor weighing fifteen or twenty pounds goes on to greater distention than a cyst containing three or four ounces. That is just the condition found in the cases under consideration. When the ducts are distended they are put upon a stretch, and when you come to make a comparison of healthy common ducts with distended ducts, you fall short. We are operating for a diseased condition.

As to inability to force stones through the common duct, I will say in one case, in which I removed twenty-eight stones, I found the same in the common duct. I forced six smaller stones down through the common duct into the duodenum, and got them afterward in the stools.

As to the method of crushing stones in the cystic or common duct, I have never resorted to it, for the reason I have been able to remove the obstruction without it.

I selected the case I have reported for the reason that it was unique. Symptoms of hepatic colic were not present; there was no marked condition of jaundice. For a long time, however, there were clay- or putty-colored stools, and with it a loss of flesh.

TUBAL PREGNANCY.

By Joseph Price, M.D.,
Philadelphia.

The study of ectopic pregnancy from a strictly operative standpoint has led me to designate this contribution by the title "Tubal Pregnancy." I do so for the reason that in all cases met by me, both in my own and in the work of others, I have in no instance met a case in which the gestation has not been primarily tubal. In one instance in which the question of original ovarian pregnancy was strongly indicated, and so stated at the time the case was reported, I have reason to believe my inferences were incorrect, as no traces of ovarian structure were found in the sac. This, of course, decides the question, if microscopic examination amounts to anything. This latter variety of ectopic gestation must be conceded as possible; though, as it is not discovered at all in these days of frequent operation by the numerous investigators in the field, it must be regarded rather as a possibility than as a probability. As to the causes of aberrant gestation, we are to consider them both as anatomical and moral. They may have their origin in anatomical loss of structure or in perversion of function, such as absent ciliary motion in the epithelium, or in absolute disease of the tube, or, as I have had more than once called to my attention, in the fright of illegitimate conception. A salpingitis from any cause may predispose to the accident, so that it is not necessary to limit its causation in this repect to gonorrhœal salpingitis alone. There are no reliable statistics as to the frequency of tubal pregnancy. Such data as we have at present depend entirely upon the

more or less unreliable observation of the past, and have very little value. Indeed, it is a question whether such statistics can ever be computed with any degree of accuracy, and for obvious reasons. As long as we have one set of observers contending for the pathological presence of simple hæmatocele, while another set ascribes all these cases to tubal pregnancy which has ruptured, so long it must be evident that for this reason, without seeking further for other source of inaccuracy, the data must be confusing.

To show the extreme variation in the ratio of cases recorded I insert the following statistics:

Low found 1 such case in 400,000 to 500,000 deliveries.

Arneth, 1 in 3542 clinical labors.

I have observed 13 cases in about 20,000.

Bandl, of Vienna, encountered only 3 cases in 60,000.

"My experience in this direction—13 cases—is inferior to that of but few authors, and because I have been able to prove, in half of my cases, the correctness of my diagnosis by the elimination of fœtal parts—6 cases—or by autopsy." (Winckel).

"Out of 60,000 women examined in the course of seven years in the clinics of Carl Braun and Späth, of Vienna, there were but 5 cases of extra-uterine pregnancy" (Bandl: "Die Krankheiten der Tuben, etc., *Deutsche Chirurgie*, Lief. 59, 1886).

This proportion would seem to be too low; Fasola observed an equal number of cases out of only 1565 pregnancies in multiparæ who had remained for some time sterile (Fasola: *Annali di Obstet.*, Florence, 1888, x., p. 145).

Tuttle, of New York, has reported 19 cases, operated upon within a short time, and has seen 5 cases during the last four months at the Roosevelt Hospital (*Amer. Journ. of Obstet.*, November, 1891, p. 238).

My own clinical experience is section in 83 ectopic pregnancies to 8000 labors.

The occurrence of ectopic pregnancy is regarded in widely different light by the theorist and the surgeon who has learned

to deal with it practically, and who has accordingly come to understand the manifold directions in which speedy disaster may troop down upon unfortunate women subjected to this calamity. The argument that many cases get well of themselves, in the presence of the multitude of disasters possible, and in the light of the horror of some of these very recoveries, is so puerile that the surgeon of practical and positive bent cannot regard them with complacency, nor consider that those who advance them have any more authority from which to speak more positive than the vaporings of fancy.

To explain the reasons for this apparently rather wide statement the following *causes of death* may be enumerated:

Hemorrrhage;
Septicæmia;
Peritonitis;
Perforation of important viscera by bone.

Death followed from septic infection in 18 cases out of the 56 cases that died in Hecker's list of 132 extra-uterine pregnancies.[1]

	Cases.
Peritonitis	12
Operations	12
Rupture and hemorrhage	7
Fecal vomiting	2
Dropsy	1
Causes not defined	4

In Galabin's case of extra-uterine and intra-uterine gestation combined, the sac was so friable that it could not be stitched to the abdominal wall, and death was probably caused by the uterus, in the premature expulsion of its own fœtus, contracting away from extra-uterine placenta, which was attached to its surface, and so causing hemorrhage (*Obstet. Trans.*, 1882, xxiv., p. 81).

Intestinal occlusion has sometimes occurred as a result of peritoneal adhesions in ectopic gestation.

Spanton operated successfuly on a case in which symptoms

[1] Hecker, quoted by Thomas and Mundé, p. 770.

of peritonitis had existed for two months (*British Medical Journal*, January 12, 1884).

In 336 cases the causes of death were mentioned. The most important are—

	Cases.
Rupture of sac	174
Exhaustion	54
Peritonitis	24
Pregnancy	16
Intestinal obstruction	8[1]

The first of these causes—to wit, hemorrhage—must be considered as the primary one in so far as fatality is considered. I mean by this that it is the first to be considered, both from its occurrence and from its fatality. If this cause is eliminated and removed, as it may be by early operation when the case is discovered, of course all other accidents are at once set aside. This argument, it seems to me, is unanswerable from a surgical standpoint. Whatever may be adduced in the way of assertion to offset it must be in the realm of theory, where positive knowledge can never hope to gain the ascendant, for the mental organization of those who refuse to know must always be content to imagine.

The time of rupture is now to be considered.

Hecker reports 45 cases of tubal pregnancy—in 26 cases rupture occurred in the first month, in 11 cases in the third month, in 7 cases in the fourth month, and in 1 case in the fifth month.

For rupture of the sac in the early stages little or nothing was done until recent times.

In 1849 Dr. Harbert, an American surgeon, first suggested operative treatment for this terrible accident; but little attention was paid to his proposal until 1866 and 1867, when Dr. Stephen Rogers, of New York, wrote and urged operation.[2]

No headway was made, however, till Tait, in recent years, took it up and by a remarkable series of successes placed it in the front of major life-saving procedures. Up to October

[1] Parry. [2] New York Medical Record 1867, vol. ii. p. 22.

26, 1887,[1] Tait had operated for early ruptured pregnancy thirty-five times, with two deaths.[2]

When we consider the almost absolute fatality of the accident, in previous years, in cases of fulminant rupture, there is little left for argument against those who insist that the dangers of recurrent fulminant hemorrhage shall not be again encountered by a woman fortunate enough to survive one accident of the kind.

The importance of early recognition and early extirpation of extra-uterine gestation sac and contents is conclusively proven by:

1. Brown's table, quoted by Harriman:[3]

	Cases.
Rupture of extra-uterine pregnancy at 3 months (mothers died)	6
Fœtus per rectum at 3 months (mother lived)	1
Fœtus per rectum at 4½ months (mother lived)	1
Fœtus died and retained 5 months (mother lived)	1
Fœtus removed per vaginam, 6 months (mother lived)	1
Laparotomy at 6½ months (mother died)	1
Without rupture or operation, at 7 months (mothers died)	2
Without rapture or operation, at 9 months (mothers died)	3
Without rupture or operation, at 9 months (mothers lived)	3
Laparotomy for 9 months (child and mother lived)	1
Laparotomy for 9 months (child and mother died)	2
Secondary laparotomy, 9 months (mother lived)	1
Primary laparatomy, 9 months (mother died)	1
Total	24

9 recovered—*i. e.*, 37.5 per cent. 15 deaths—*i. e.*, 62.5 per cent.

2. Bond.[4] Cases operated on, child viable (1888–89): 11 cases, with 4 children saved—35.4 per cent. 11 cases, with 7 children lost—64.6 per cent. 11 cases, with 4 mothers lost —35.4 per cent. 11 cases, with 7 mothers saved—64.6 per cent. 44 cases, six to eight weeks after death of fœtus, with 9 mothers lost—20.4 per cent. 44 cases, six to eight weeks after death of fœtus, with 35 mothers recovered—79.6 per cent.

[1] British Medical Journal, November 12, 1887.

[2] Greig-Smith, p. 318, 319.

[3] Transactions New Hampshire Med. Soc., November 23, 1889.

[4] Journ. Amer. Med. Assoc., Chicago, March 21, 1891, xvi., No. 12.

3. Formad reported 35 post-mortems in which he found ruptured tubal pregnancy—31 of them had died within a few hours after the appearance of unfavorable symptoms.

4. Werth[1] gives 16 cases of advanced pregnancy of tube and broad ligament. 13 cases of tubal pregnancy, at six to ten months, without implication of any other organ. 8 cases of extra-uterine pregnancy, about at term, with a living child. 1 recovered—12.5 per cent. 7 deaths—87.5 per cent. Cause of death: Shock, 1; certainly from septic peritonitis, 5; probably from septic peritonitis, 1. 40 cases extra-uterine pregnancy in an advanced stage. 40 operations after death of fœtus, with conservative treatment of sac. 26 recovered—65 per cent. 14 deaths—35 per cent. Most operations were performed from one week to three months after death of the fœtus; in several cases the fœtus had been dead from one to two years—in 1 case, eight years.[2]

5. Harris[3] cites 30 cases of primary laparotomy with living fœtus near term.

20 *cases up to* 1880:

1 recovery of mother, *i. e.*	5	per cent.
19 deaths of mother, "	95	"
10 successes for child "	50	"
10 deaths for child "	50	"

10 *cases between* 1880 *and* 1888:

4 recoveries of mother, *i. e.*	40	per cent.
6 deaths of mother, "	60	"
6 recoveries for child, "	60	"
4 deaths for child, "	40	"

"Should pregnancy unquestionably be abdominal, as proved by its advance beyond period ordinarily advocated for tubal distention, and by the comparatively small size of uterus, it should not be interfered with until completion of full term. At that time an effort of labor usually occurs and gives a signal of action."

[1] Kiel, 1887.
[2] Trans. Gynec. Soc., Chicago, October 19, 1890.
[3] Amer. Journ. Med. Sci., September, 1888.

"Should this most fortunate event occur and be recognized, the crowning triumph of obstetrical surgery may be reached in the delivery of a living child."

Cases of this kind have been reported by: Friedreich, 2; Kœberle, 1; Fournier, 1; Rennert, 1; Price, 4.

6. In addition to these, Winckel presented 5 before the Gynecological Society of Munich in the summer of 1887, and since then has observed a sixth case. Of these 11 cases 3 were fatal—1 of Fournier's and another which was treated by the author with morphine for the second time.

Operated for dead fœtus after spurious labor.—Litzmann has collected 33 cases—24 occurring between 1870 and 1880; there were 19 recoveries and 14 deaths.[1] Hoffman, 1.

Breudel[2] operated successfully on a case where there was absolute constipation for four weeks. In this case fœtus was not decomposed; placenta was almost separated.

Notta[3] records a case on which M. Boilly operated, and in which intestinal obstruction was present. Fœtus had been carried for eight years, and cyst was intimately adherent. A loop of intestine was found strangulated, and the constriction was divided. Patient died, and after death a second strangulation was found. A preparation in the Bristol Infirmary Museum shows a strangulation of a double loop of bowel by adhesions around a gestation sac.

Abdominal section, with a mortality of 100 per cent., in 1841 (Campbell) will be shown to have progressively a much better prognosis the nearer we come to the present time. In 1880 Litzmann gave a series of 43 cases, with 23 maternal deaths—a mortality of 53 per cent. His statistics in detail are:

10 sections with living children, 9 deaths—90 per cent.; 33 sections with dead children, of which 10 were performed one to five weeks after death of child, with 8 deaths, or 80

[1] Ed. Med. Journ., February, 1884, p. 697.
[2] Centralbl. f. Gyn., October 13, 1883, p. 649.
[3] Prog. Méd., 1884, xii., 196.

per cent.; and 23 performed from six weeks to a year after the death of the child, with only 6 deaths, or 26 per cent.

In 1889 Leopold Meyer, of Copenhagen, in his annual compilation and summary, collected the operations of the previous year (1888)—24 sections, 8 maternal deaths, 33 per cent.

The same author, in his summary of 1889, gives the sections for 1889 as 35, with 6 maternal deaths.

Mortality from all sections late in pregnancy of only 17 per cent.

It will thus be seen that section for extra-uterine pregnancy at or near term, irrespective of the condition of the placenta and child, has the enormous decrease in maternal mortality from about 100 per cent., in 1841, to 17 per cent., in 1889.

Of 52 cases of section performed at varying periods after death of fœtus, 37 recovered and 15 died.

Of the fatal cases, only 3—all cases of free hemorrhage—could be attributed to the operation. In all the others the cases were almost helpless at the time of the operation. As Lusk remarks: "The resources of surgery are rarely successful when practised on the dying."

In the early stages, and before rupture, abdominal section ought to be a very simple and successful proceeding.

The size of fœtus increases the magnitude of operation. If, as is likely, operation will be called for at the end of nine months, why wait for that time when the dangers are so much greater?

It seems evident, past argument, from the above gathered records of operation, that the consensus of opinion by those competent to speak from results must be for early operation. But there are, in addition, those cases to be considered in which, after primary rupture, the fœtus has still lived and advanced to full term. Here comes in an altogether different question for our consideration. It is the question of operation with a view of saving both the life of the mother and that of the child. If one is to be lost, my view coincides with those who believe that the life of the mother is paramount and that that

of the child is of secondary consideration. What constitutes the chief danger to the mother in the operation at term in tubal pregnancy is the removal or accidental detachment of the placenta. It is easy enough to remove the child and save it, if it is viable, by operating at or near term ; but the danger of fatal hemorrhage from vascular walls that cannot contract, as do the uterine structures, is the vital question of the operation, so far as the mother is concerned. If we do not remove the placenta the risk of septic infection still remains, so that we are still between the two horns of the dilemma.

My own opinion is that if the placenta can be removed without damage to other structures, and without the causing of too great hemorrhage, it should not be left. I have seen it so located that this could be done. On the other hand, I have seen it, as in a case reported by me, where it could not be accomplished without disastrous bleeding. The question of stitching the sac to the abdominal opening does not seem to have much to recommend it, as witness the following statistics :

Stitching sac to incision, 20 cases : 13 complete removals of sac, 65 per cent. ; 7 sac stitched to incision, 35 per cent.

13 complete removals of sac : 10 recoveries, 77 per cent. ; 3 deaths, 23 per cent.

Sac stitched to incision, 7 cases : 7 recoveries, 100 per cent. ; no deaths. 20 cases : 17 recoveries, 85 per cent. ; 3 deaths, 15 per cent.

Of 40 cases of operation in which sac was treated conservatively : 26 recoveries, 65 per cent. ; 14 deaths, 35 per cent.[1]

While, theoretically, the idea of allowing an ectopic pregnancy to go on to full term may be very enticing, it would appear that, from a practical point of view, the gain offers little compared with the risk. I have grown to the habit of thinking that in questions such as these we should bring, as another writer has expressed it, the argument home—that is, what would we do or have done in our individual families ?

[1] See Trans. Gyn. Soc., Chicago, October 19, 1890.

Experimental surgery is good enough in its way outside of humanity, but humanity and life must be paramount. We may argue to save the life of the child, but if we lose that of the mother what have we profited? We may agree that the life of the child may be saved, but we must also agree that the life of the mother may be lost. So where are we in the argument?

The following data, introduced to show comparatively the advances made in the surgery of this affection after a period of five months has been reached, will be interesting. It must be premised, however, that the variations in each individual case make it impossible to decide whether the analogical method of reasoning can be applied to the surgery of ectopic gestation. Later results are better than the earlier. This can be explained by the general improvement in all the methods and technique of abdominal surgery. The question is still open whether the results can be so much further improved as to argue for an invariable procedure. However this may be, we must acknowledge that the results have not thus far been as a whole encouraging.

Maygrier,[1] out of 17 cases collected up to 1886, found a mortality of 15 cases, or 88 per cent. In 10 cases women died of hemorrhage, either at time of operation from detachment of adherent placenta, or later, following the spontaneous detachments of placental fragments. Of the infants, 9 lived only a few hours, and the fate of 8 was unknown.

Werth[2] has collected 8 cases, published from 1880 to 1886, with 7 deaths for mother and 3 only for child; 2, however, succumbed soon after birth; 2 others were well at the age of 3 months.[3]

Harris has still more recently collected 30 cases of primary sections performed; that is to say, before death of fœtus. He

[1] "Terminaisons et Traitement de la Grossesse Extra-utérine," Thèse d'Agr., Paris, 1886.

[2] Loc cit. (23), p. 112.

[3] Normann: Norsk. Magaz. f. Lægevidensk, 1880, Band x. Netzel: Hygeia, April, 1881.

found, up to 1880, 20 cases, with 1 success only for mother and 10 for child; life persisting for a variable period. From 1880 to 1886 he found 10 cases, with 4 successes for mother and 6 for child.[1]

At the present time the aspect of the question is changed. Operations are now, as a rule, successful, as witness the following statistics gathered since those of Werth, in 1886:

Lazarewicz,[2] of Kharkoff: Total extirpation of sac. Woman recovered; child lived twenty-one days.

Breisky:[3] Operation in eighth month of a tubal intra-ligamentous pregnancy. Complete extraction of sac and placenta; rapid recovery of mother. Child was perfectly viable; died three weeks later of phlebitis of umbilical vein.

John Williams:[4] Sac was not extirpated, but drained.

Eastman,[5] of Indiana: An intra-ligamentous pregnancy of eight months without rupture of tube. Total extirpation of sac; irrigation, drainage; recovery. Child was well formed and vigorous.

Olshausen:[6] Operation ten days before full term. Tubal pregnancy transformed into an abdominal pregnancy by rupture of sac, without hemorrhage, six days previously. Child free in peritoneal cavity. Extirpation of placenta and remnants of sac. Easy operation.

Braun von Fernwald:[7] Abdominal pregnancy. Placenta was fixed in Douglas' pouch, which was invested with a thick membrane, the only vestige of sac which also covered posterior portion of uterus and broad ligament. Large vessels had to be tied; these extended from ileac meso-colon to pla-

[1] "Extra-uterine Pregnancy Treated by Cystectomy," etc., Amer. Journ. Med. Sci., August and September, 1888.

[2] Wratsch, St. Petersburg, 1886, vii. 76–115; abstract in Répertoire univ. d'Obstét. et de Gyn., July, 1886, p. 277.

[3] Wien. med. Presse, 1887, No. 48.

[4] Obst. Trans., London, 1883, p. 482.

[5] Amer. Journ. Obst., September, 1888, xxi., p. 929.

[6] Gesellsch. f. Geb. und Gynäk. zu Berlin, Nov. 9, 1888; Centralbl. f. Gyn., 1888, No. 49, p. 811.

[7] Obst. and Gyn. Soc. of Vienna, March 26, 1889; Centralbl. f. Gyn., 1889, No. 36.

centa, which was then detached. Hemorrhage necessitated elastic ligature of uterus; hysterectomy and tamponing peritoneum with iodoform gauze. Slow recovery of mother. Child died twelve hours after operation of capillary bronchitis, ascribed to inspiration of liquor amnii.

Treub:[1] Ovarian, or tubo-ovarian, pregnancy; operation three weeks before term. Partial resection of sac, which was intimately adherent to abdominal wall; extraction of placenta; tamponing of peritoneum with iodoform gauze. Uninterrupted recovery. Child living and thriving.

These men have all operated a little before term, or at term, and have saved both mother and child. The last named, out of three operations, saved all the children and two of the women.

Champneys[2] saved child only.

Joseph Price[3] lost both; died two weeks after operation from hemorrhage; but he operated in the presence of peritonitis, due to rupture of sac.

Hildebrandt[4] operated upon two moribund cases, but succeeded in saving one child.

G. Beisone[5] lost the mother, but saved the child. Excluding cases of Price and Hildebrandt, which were desperate, we have as a result of 13 operations, 9 living women and 11 children who lived for at least a few days.

M. Price's one case. Mother and child saved; operation after subsidence of spurious labor and beyond term.[6]

Whatever the verdict may be ultimately as to the time of operating, the method and site for incision seem to be fixed. The method of evacuating the liquor amnii has been followed by such doubtful and calamitous results that it is now practically abandoned. The vaginal method of extirpation of the sac is also now almost universally condemned, and can only

[1] Zeitschrift f. Geb. und Gynäk., 1888, xv., Heft 2.

[2] Brit. Med. Journ., December 3, 1887.

[3] Communication to Harris, Amer. Journ. Med. Sci., Sept. 1888, p. 264.

[4] Berliner klin. Woch., July 20, 1885, p. 465.

[5] Gazetta Medica di Torino, 1881, xxxii., p. 553.

[6] Pozzi, pp. 256, 257.

be thought of in the rarest possible conditions. I can do no better than to quote Dr. Herman's conclusions, which are as follows:

1. The operation of opening an extra-uterine gestation sac by the vagina early in pregnancy, before rupture has taken place, by the cautery, knife, or otherwise, is a dangerous and unscientific procedure. Abdominal section ought always to be preferred to this.

2. Soon after rupture has taken place, when interference is called for to arrest hemorrhage, abdominal section is more likely to succeed than vaginal.

3. When rupture has taken place, and the effusion of blood is followed by pyrexia, the indications for incision of vagina are the same as those in hæmatocele from any other cause.

4. At or soon after full term, before suppuration has taken place, there may be conditions which indicate delivery by the vagina as preferable to abdominal section. These are:

5. When the fœtus is presenting with the head, breech, or feet, so that it can be extracted without altering its condition.

6. When it is quite certain, from the thinness of the structures separating the presenting part from the vaginal canal, that the placenta is not implanted on this side of the sac, and it is not certain that the placenta is not implanted on the anterior abdominal wall.

7. If the child cannot be delivered by the vagina without being turned, abdominal section should be performed.[1]

Outside of operation it is often a question of interest to decide what the record of recovery is in these cases. This is a matter of curiosity rather than of positive scientific inquiry. With the same degree of reason we could inquire, How many men that cannot swim are not drowned? How many fires without the assistance of the fire department are not general? How many ships reach harbor without a pilot?

It is rather of greater importance to look at the records of these cases before the legitimacy of operation was thought of,

[1] Tait: Dis. of Wom. and Abd. Surgery, vol. i. p. 519.

as suggested by Stephen Rogers, and compare the mortality then with the recovery now. Such a comparison will leave no doubt as to which side of the question has the vantage ground of the argument. In some notes on the subject I have put this inquiry under the head of frequency of recovery. I had better placed it under the head of frequency of fatal termination. Under this head I append the following:

Hecker found that out of 132 cases of abdominal pregnancy 72 terminated in recovery. Recovered: 28 cases after expulsion of fœtus *per anum*, 17 cases after formation of lithopædion, 15 cases after elimination through abdominal wall, 11 cases after section, 3 cases following vaginal section, 2 cases undefined causes.[1]

Whatever be the variety, the period, or the circumstances connected with this vice of gestation, the prognosis is bad. True, a large number of women escape death; but this fact does not contradict the statement just made. The prognosis is most favorable in abdominal pregnancy when adhesion has occurred, from death of the fœtus and subsequent inflammation between the sac wall and the parietal peritoneum; less favorable where no such adhesion exists and the peritoneal cavity is free in front of the fœtal shell. It is less favorable in interstitial than in tubal pregnancy, and least favorable if the fœtus be living than if it be dead.

Kiwisch reported 100 cases of extra-uterine pregnancy with 18 recoveries.[2]

Puech: 100 cases of tubal pregnancy—98 cases rupture of tube; 2 cases rupture of vein of broad ligament, 1 recovery. 199 cases of elimination of fœtus in ovarian and abdominal form, 146 recoveries, 53 deaths.[3]

Prognosis.—This differs according to individual cases. The most frequent form of tubal pregnancy seems also to be the most dangerous; in the interstitial form rupture always (?) occurred (Cauwenberghe, 33; Hecker, 26 cases); in the strictly

[1] Thomas and Mundé, p. 770.

[2] Spiegelberg: Lehrbuch der Geburtshülfe, 1877, p. 323.

[3] See Courty, p. 996.

tubal form in five-eighths of the cases (Cauwenberghe, 46, with 33 fatal cases; Fraenkel, 42, with 28 fatal).

Rupture with fatal hemorrhage is the most frequent termination; pyæmia, septicæmia, and peritonitis are much rarer.

Recovery took place in tubal pregnancy, according to Hennig, in 4.6 per cent. of the cases which were left to Nature, while of those which were treated 7.3 per cent. recovered.

Abdominal pregnancy, according to Cauwenberghe, is the most favorable; for, of 128 cases, gravidity was usually prolonged in 85, 58 of which recovered, while 29 died. But for all that, we should not overlook the fact that the cases which took the form of pelvic hæmatocele or subperitoneal hæmatoma, and terminated favorably, could not be included in the calculation, and their omission renders the prognosis much worse. This will help to explain such statements as those of Puech, according to whom, of 100 tubal pregnancies, rupture occurred in 98, of which 97 were fatal, while 2 others died from causes other than rupture.[1]

Concerning the prognosis of such cases Goupil says: "It is but true, I fear, that we are authorized in saying that all the cases of intra-peritoneal hemorrhage arising from extra-uterine pregnancy end in death, and although death has been delayed for six months (as in the case already quoted), it is wholly exceptional. This was absolutely true in my own experience till I was emboldened—till, I say it—till I was shamed by Mr. Hallwright's case into opening the abdomen and saving their lives."

We come to the following conclusions: That in the great majority of cases of extra-uterine pelvic hæmatocele, even when due to ectopic pregnancy, the disease may generally be left alone, being rarely fatal, and that it is to be interfered with only when suppuration or extreme hemorrhage has occurred. That, on the contrary, intra-peritoneal hæmatocele is fatal with such almost uniform certainty that so soon as it is

[1] Winckel.

suspected the abdomen must be opened and the hemorrhage arrested. In the overwhelming majority of cases the source of the hæmatocele will be found in the broad ligament, and then it can be dealt with and with every prospect of success.[1]

Hecker[2] found the fœtus expelled through the rectum in 28 out of 132—that is, in 20 per cent. of extra-uterine pregnancies.

Up to the end of 1875, Parry, for his classical work on extra-uterine pregnancy, had collected a list of 62 operations for the removal of extra-uterine children, with the encouraging result of 30 successes and 32 failures.[3]

Parry's statistics of 500 cases of extra-uterine fœtation give a mortality of 67.2 per cent.

As a result of operation a living mother and a living child can be credited only to seven surgeons—Jessop, of Leeds; A. Martin, of Berlin; Eastman,[4] of Indiana; Breisky;[5] Lawson Tait; Olshausen;[6] M. Price, 1892.

Perhaps a case of Braun-Fernwald,[7] in which the child died of inspiration pneumonia, should be added to the successes.[8]

Spontaneous recovery has been most often effected through the elimination of cyst by suppuration, and this process is less or more serious according as it is or is not methodically and antiseptically treated. The results are known in 499 out of 500 cases collected by Parry. In 336, patient succumbed; in 163 she recovered, giving a general mortality of 67.2 per cent.[9]

Of the obsolete methods of treatment and with electricity I shall not here take time to deal. Suffice to say that these are no longer considered by the practical surgeon, and are left

[1] Lawson Tait: Dis. of Wom. and Abd. Surg., vol. i. pp. 475, 476.

[2] Bandl.

[3] Greig-Smith.

[4] Amer. Journ. Obstet., October, 1888.

[5] Wien. med. Presse, xlviii., 1887.

[6] Abstract from Prager med. Woch., 1890, No. 8; Amer. Journ. Med. Sci., August, 1890.

[7] Arch. f. Gynäk., xxxvii. 2.

[8] Greig-Smith.

[9] Pozzi, p. 252.

to those who either are without surgical skill, dwell apart from it, or cannot comprehend it.

Another question of practical importance in the consideration of this subject is that of the liability of recurrence in the other side after a primary operation. To this inquiry it would seem that only an inferential reply can be given, and that is that if the conditions are such as may repeat themselves by reason of existing or probable disease, then the condition may again recur. This is in reality saying very little. Recurrence has been noticed frequently enough to justify a suspicion of further accident, in proof of which the following cases may be cited :

Reed (C. A. L.) reports, quoting Meyer, 11 cases—Herman, Olshausen, Veit, Tait—11 operations, 11 recoveries, 11 recurrences, 11 re-operations.[1]

A number of more recent cases could be reported in the works of Sutton, Price, and others.

Should the appendages of the unaffected side be removed in each case of unilateral ectopic conception ?

The interval between the two extra-uterine conceptions in Herman's case was from January, 1887, to May, 1890 ; in Tait's case fifteen months.

Extra-uterine pregnancy may recur repeatedly in the same woman.[2]

Thus Tait[3] has recorded the case of a patient who died of rupture of a tubal pregnancy on one side some time after successful operation for a similar condition on opposite side.

The possibility of primary pregnancy cannot be denied in the face of such evidence as is afforded by cases of impregnation through abnormal orifices in uterus.

Two such cases are recorded by Lecluyse[4] and by Koeberlé —one through the canal left after hysterectomy for myoma.

[1] See Amer. Journ. Obstet., xxiv., No. 2.

[2] Case of Siegenbeck von Heukelom of bilateral tubal pregnancy.

[3] Brit. Med. Journ, 1888, i. 1001.

[4] Bull. de l'Acad. de Méd. Belgique, 1869.

Some doubt has recently been thrown on the reality of Koeberlé's case.

A case of Keller's is noted by Spiegelberg in which abdominal pregnancy occurred two years after an almost complete hysterectomy.[1]

Herman,[2] three years after removing a ruptured tubal pregnancy on one side, diagnosed and removed before rupture a tubal pregnancy of the other side.

Veit[3] records three examples in his own practice.

Olshausen[4] successfully removed a living child from a right tubal pregnancy in a patient on whom he had previously operated for left tubal pregnancy.

Kussmaul holds that many cases of so-called tubal pregnancy are really instances of gestation in rudimentary horn of a bicorned uterus. He collected 13 cases of this sort, all of which died of rupture between fourth and sixth months.

Parry and others consider that Kussmaul overestimates the frequency of cornual gestation; and, even if it were more common than is generally supposed, it need not result in rupture.

From an operative standpoint, as opposed to the theoretical vaporing on this subject, it is interesting to look at a few figures. This is the aim of my paper at this time. The figures are not picked, and, I think, will be more interesting on this account.

They are scattered cases reported in 1890, 1891, and 1892: 186 extra-uterine gestations, 114 recoveries, *i. e.*, 60.7, 49–93 per cent.; 72 deaths, *i. e.*, 39.2, 44–93 per cent.

Of the 186 cases 119 submitted to operation, *i. e.*, 63.44 per cent.; 67 were subjected to expectant treatment, 36.56 per cent.

Of the 119 operations (abdominal sections): 97 recoveries, *i. e.*, 81.51 per cent.; 22 deaths, *i. e.*, 18.49 per cent.

[1] Greig-Smith, p. 321. [2] Brit. Med. Journ.

[3] Zeit. f. Geb. u. Gyn., xvii. 335.

[4] Abstract from Prager med. Woch., 1890, No. 8, in Amer. Journ. Med. Sci., August, 1890.

Of the 67 not operated upon 17 recovered, *i. e.*, 25.37 per cent. Most of these were purely speculative cases; no verification of the diagnosis except in 3 cases. These 17 cannot properly be classed as recoveries, because in all except 2 a mass remained. Fifty deaths, *i. e.*, 74 = 63 per cent. The diagnosis proved that in 34 instances, *i. e.*, 68 per cent., by autopsies, discharge of fœtal parts, etc.; in 16 cases, *i. e.*, 32 per cent., the fatality due to delay after recognition, tinkering, late recognition.

After this somewhat extensive consideration of the operation of ectopic gestation from the standpoint of others, it is my firm conviction, fortified by my own experience, counting now eighty-three cases with three deaths, that the operative treatment is the only one to be considered. I am fully convinced also that these pregnancies are rarely, if ever, in the broad ligament, and that the frozen sections of Hart are misleading. In the case of fœtus gone to term, in my own direct and indirect experience, the child has in no instance been in the broad ligament. I regard the chief danger of the operation as that of hemorrhage. If the patient is found so weak as to render operation an almost certain failure, I resort to salt-water transfusion in order to restore the arterial tension. From an operative standpoint I consider the success of surgical interference in this disease of women as possibly the most prominent innovation of the surgery of modern times. I present typical cases:

Case No. 79.—Mrs. W. was seen in consultation with Dr. John Musser. She was in collapse from the fifth or sixth hemorrhage following rupture. Exsanguine, face livid, extremities cold, restless, sighing respiration, pulseless at wrist. We decided upon transfusion, free use of strychnine, digitalis, and champagne, followed by section, removal of tubal sac, placenta, fœtus, and an enormous amount of blood and clot; irrigation and drainage; speedy recovery.

Case No. 80.—Mrs. B., white, aged twenty-nine; married nine years; sterile; absence of three periods followed by acute, agonizing pelvic pains, marked symptoms of concealed

hemorrhage, subjective and objective symptoms of ruptured tubal pregnancy; section; removal of ruptured tube, placenta, and fœtus, and a large quantity of clotted blood; irrigation and drainage; recovery.

It is better to act, and that promptly, than to stand before these cases, paralyzed by uncertainty as to what to do. All of us are sensible of the inadequacy of many of our methods to meet all contingencies. We use them because they are the best known to us, and give us the best results. We are hopeful for and will welcome better. The steps of procedure in these cases are clear and should be completed at any risk. The bulky, lowly organized placenta should be removed—should never be permitted to remain to poison the patient, which it does in every case where it is permitted to remain. It is better to contend primarily with the loss of blood than later with overwhelming sepsis. There are always risks to the mother after primary rupture—those of peritonitis, universal adhesion, secondary or recurring hemorrhages, suppuration of fœtus, placenta, and clot. All periods of rupture are favorable to successful surgical interference. An element of the history connected with these cases is that few of them are kept under observation with the definite purpose of removing the viable child at the period of spurious labor; but few are recognized at that time; alarming symptoms develop and subside, and consultation with a specialist, if they are at any time consulted, follows the death of the child—it rarely antedates it. Then all the conditions are found greatly aggravated by delay or neglect, or that which is infinitely worse than either or both—inexcusable ignorance.

Where there is doubt there should be consultation—an honorable and manly acknowledgment of individual limitations. I should feel it well worth my while to give utterance to my opinions in these cases in tones so unequivocal, in words of such strong and deep emphasis, so barbed that they would deep plant the one all-vital lesson—that they are grave, perilous troubles from the very instant of their inception, and from the instant of their first recognition there should be no

suspension of the vigil over them. They burden every physician into whose hands they fall with untold anxieties and doubts. Consultations for suspected extra-uterine pregnancy are quite common in those peculiar cases of much-attenuated uterine walls in normal gestation, but the ectopic cases are permitted to pass through the primary rupture, recurring ruptures, almost constant pain, and spurious labor, entailing impaired general health without even a suspicion of the patient's peril. Some ten cases in my own experience have had the above history. Tubal pregnancy is dangerous throughout its existence. The subject is never safe until surgically relieved, and the time for this relief is when the trouble is first recognized. *Exceptionally*, is the trouble recognized before rupture. The few cases recorded have all been by men with one experience, with very limited knowledge of the murderous troubles found in the pelvis. A few surgeons, with an experience of nearly one hundred sections for ruptured tubal pregnancy, have never found one unruptured.

The old and non-surgical rule of leaving the placenta to slough away is too dangerous and prolonged to be practised. The placenta should be removed in every case, or washed and hermetically sealed, thus favoring its healthy digestion and avoiding gangrenous separation and detachment. Good surgery must settle the few remaining points. Secondary rupture of broad ligament, discharge of placenta and fresh adhesions, or the second implantation or grafting of the placenta, have never occurred in my experience; nor have I any knowledge of such cases except that conveyed through the literature of the subject, where they appear in large numbers. Basing the conclusions of my judgment upon my own clinical experience, I must hold to the tubal origin and the intra-peritoneal rupture, and that all that follows tubal rupture is within the pelvis and peritoneal cavity, and not within the leaflets of the peritoneum forming the broad ligament. The risk of removal is that of hemorrhage, which the speedy and thorough use of our present good methods will avoid—firm sponge or gauze

packs, the use of heat or solution of iron. I will add here that iron will favor fecal fistula and tedious convalescence. Ligatures and forceps cannot be used about viscera with safety; drainage with pressure will be the safest and surest. In the matter of decision as to methods, such cases are not of the kind in which you can indulge in indecision; they will brook no delay in your determining what to do and how you will do it. If there be present a bulky placenta, living and growing, either before or after the death of the fœtus, the choice of one of two methods must be made and practised with rapidity and courage, cleaning and hermetically sealing the placenta and abdomen, trusting to absorption or secondary operation for its removal in the event of its behaving badly. The removal of a growing and almost universally attached placenta is one of the most startling and difficult procedures in surgery; it taxes the toughest courage. The hemorrhage is profuse and alarming, and sometimes uncontrollable; the contraction of all tissues to which it is attached simulates very much that of uterine tissue. Rapid separation, heat, and firm pressure will commonly succeed in controlling it. As to the choice of time for operation, after a careful study of the history, surgical procedure, and results in the recorded cases, united with my own experience, I am strong in the conviction that there is but one safe choice, and that is prompt removal when the accident is first recognized Delays at any period of ectopic pregnancy are dangerous. I scarcely think it is wise to risk the mother's life for a living child by waiting for viability. In this view of these cases we are aware that many very skilful and conscientious surgeons differ with us; they defend their side from what they believe to be a moral and humane standpoint, and no one of them, to our knowledge, defends his position from a sound surgical standpoint. We rather take it that theirs is the sentimental side of the question. Good surgery is very intolerant of sentiment.

I cannot quote a more reliable authority on this subject of diagnosticating extra-uterine gestation prior to the period of rupture than Mr. Tait, whose phenomenal experience and

success entitle him to speak with authority. In replying to a statement as to his own utterances, he said: "I have never said that extra-uterine gestation had never been recognized prior to the period of rupture. What I did say was that I have never recognized them. There was a very good reason for that, for, with one exception, he had not seen but one until rupture had taken place, and in that one case he had mistaken it for something else. I have now seen ten post-mortem examinations or surgical operations performed on over eighty cases of intra-peritoneal hæmatocele, and in every one of them the cause was ruptured tubal pregnancy." Dr. Berry Hart, who has given the subject very patient and studied investigation, and discusses it from a high surgical standpoint, is worth quoting here: "As all know, the Fallopian tube is in the vast majority of instances the starting-point of extra-uterine gestation; the most common result of this is that rupture occurs usually at the second month through some part of the tube covered by peritoneum—a result almost universally fatal if left alone, and as invariably curable if operated on in time by abdominal section."

Summary of Cases (not including my own):

		Per cent.
247 operative cases—		
	206 recoveries	= 82.75
	41 deaths	= 17.25
132 palliative and expectant cases—		
	62 recoveries	= 46.97
	70 deaths	= 50.03
102 cases—		
	21 no operation; no treatment; fœtus *quiescent* throughout life	= 20.58

		Cases.
112 cases—		
	Section operation	112
	Expectant treatment	0
186 cases—		
	Section operation	119
	Expectant treatment	67

102 cases—	Cases.
Section operation	16
Expectant treatment	65
No treatment	21

400 cases—	Per cent.
247 section operations	= 61.75
132 expectant treatment	= 33.
21 no treatment; quiescent	= 5.25

Price's Cases:

83 cases—	
80 recoveries	= 96.22
3 deaths	= 3.61

These added to other operative cases:

326 operative cases—	
282 recoveries	= 86.51
44 deaths	= 13.49

A CONTRIBUTION TO THE STUDY OF ABDOMINAL PREGNANCY.

By Henry C. Coe, M.D.,
New York.

In spite of the amount of recent literature bearing on the subject of ectopic gestation, our knowledge of this interesting abnormality has been widened almost entirely in the direction of early tubal pregnancy. Unfortunately, the same attention has not been bestowed upon the later stages, especially upon that described as secondary abdominal pregnancy, because the opportunities for studying it at the operating-table and post-mortem are limited. The consensus of opinion is now against the existence of primary abdominal gestation, and we may assume that (excluding a few doubtful cases of the ovarian variety) every ectopic pregnancy is originally tubal. The fact that even the secondary form of abdominal gestation is still denied by competent observers, and that many of the reported cases have been described so inaccurately as to make them comparatively valueless, renders it important that every authentic case should be recorded as fully as possible. In the case reported the history was so unusual as to render it quite interesting, while the opportunities for studying the anatomy of the condition were as favorable as could be secured outside of the dead-house:

Mrs. A., thirty-eight years old, is the mother of three living children, her labors having been normal. She had never had any previous pelvic trouble. She menstruated in April, 1891, skipped the following period, and on June 26th began to have what she described as labor-pains, accompanied by profuse

metrorrhagia, which continued for twelve days. The patient passed several large clots, and supposed that she had aborted. She recovered, and there was no further flow until September 22d, when she was first examined by Dr. S. Marx, a skilled obstetrician, who found that the uterus presented the ordinary condition of the pregnant organ at the beginning of the fourth month, as to size, elasticity and softening of the cervix. Intermittent contractions could apparently be felt. The patient had morning sickness, with enlargement and tenderness of the breasts, and a few days later she affirmed that she felt quickening. There seemed to be no reason to doubt the existence of normal pregnancy. On November 5th she was seized with severe pains in the right inguinal region, and had the same profuse flow as before, without any assignable cause. The hemorrhage continued for several days, but without the uterine contractions that had been present on the former occasion. There was no collapse or other evidence of internal bleeding. A mild attack of peritonitis, localized on the right side, followed and lasted for two weeks, the temperature seldom exceeding 100° F.

The patient made such a good recovery that she was soon able to resume her household duties, being entirely free from pain. In the meantime the supposed uterine tumor continued to enlarge regularly and symmetrically, corresponding to the normal increase in size of the pregnant organ, and the patient insisted that she could feel fœtal movements, though her physician was never able (after repeated attempts) to hear either these or the heartbeat. By external palpation he could detect fœtal parts, though internal ballottement could not be obtained.

I saw the patient in consultation April 19th. She was in good general condition and had no pain. She still insisted that she felt life. The breasts were flabby, with retracted nipples. The abdomen was symmetrically enlarged, presenting the appearance ordinarily observed at the end of pregnancy. On making external palpation I felt a median, thin-walled sac, filled with fluid, in tapping upon which the characteristic sensation given by the small parts of a fœtus was clearly appreciable. There were no intermittent contractions in the sac, and auscultation was entirely negative. On making a vaginal examination I noted the fact that the cervix was hard, but thought that this might be explained by the presence of an old laceration, with extensive in-

duration. The cervix was continuous with the abdominal tumor, and moved with it; it was impossible to separate the body of the uterus from the enlargement. There seemed to be no reason for passing a sound. The tumor bulged posteriorly into Douglas's pouch, but the sensation was rather boggy than fluctuating. Internal ballottement could not be obtained. Nothing abnormal could be detected in either ovarian region. The examination was unaccompanied by pain or discomfort.

In spite of the somewhat peculiar history, and the impossibility of determining when conception occurred, the case seemed to be one of intra-uterine pregnancy at term. I made a probable diagnosis of hydramnios, with death of the fœtus, and saw no reason for immediate interference.

I did not hear from the patient again until the middle of May, a month later, when her physician informed me that she was still *in statu quo*. As she had long passed the normal period of pregnancy, and her abdomen had continued to enlarge from the increased accumulation of fluid, he began to think that the condition must be either ovarian cyst or abdominal pregnancy. She was admitted to the hospital, and it was evident that a marked change had taken place, both locally and generally. She had become much emaciated, presenting the appearance of a patient with malignant disease. The abdomen was much distended, with general fluctuation, and the superficial veins were prominent, as in cancerous ascites. A thorough examination was made under anæsthesia. External ballottement was obtained as before, but less clearly. Fluctuation was general, but there was still to be felt a thinned-walled median sac, filling the entire abdominal cavity, displacing the stomach and liver upward, and bulging downward into Douglas's pouch, where distinct fluctuation was obtained, the wave being transmitted from the abdomen. The corpus uteri was merged with the sac, but a sound was introduced anteriorly to the depth of four inches, proving that the uterus was probably empty. Normal pregnancy was thus positively excluded, and the diagnosis lay between abdominal pregnancy, ovarian cystoma, or, possibly, malignant disease of the peritoneum with encysted ascites. The possibility of intra-uterine gestation as a complication of an abnormal condition (ovarian cyst or malignant disease) was also borne in mind, as well as the existence of a former tubal gestation with rupture, as indicated by

the history. The patient was examined by several of my colleagues, who inclined to the diagnosis of multilocular ovarian cystoma.

Operation, May 25, 1892. The usual median incision was made. The peritoneum was found to be much thickened and fused with the wall of the sac, the latter being firmly adherent to the abdominal wall. At least a gallon of chocolate-colored fluid was evacuated. A fully developed fœtus was found in the upper part of the sac, its head resting against the under surface of the liver. The fœtus was removed and the cord ligated. The placenta was attached to the entire posterior surface of the uterus, filling the cul-de-sac. It was bloodless, and was readily separated without hemorrhage. The pelvic as well as abdominal viscera were so completely shut off from the sac that it was not deemed advisable to attempt any extensive detachment of the latter for the sake of studying its relations. The following points were, however, clearly determined: The sac was symmetrical with respect to the uterus (which was moderately enlarged and lay in front of and below it), being firmly adherent to the entire posterior surface of the organ, and to the posterior aspects of both broad ligaments, dipping down to the bottom of the cul-de-sac. So far as could be determined by palpation, the adnexa were not enlarged. The abdominal viscera were displaced upward, but whether extensive intestinal adhesions were present or not, it was impossible to determine. The wall of the sac had a uniform thickness of an eighth of an inch, and was lined with a cheesy, disintegrated mass, evidently the result of degenerative changes.

Its cavity was thoroughly irrigated and, on account of slight general oozing, was packed with iodoform gauze, a drainage-tube being introduced to the bottom of Douglas's pouch. The edges of the sac were stitched into the abdominal wound, a large opening being left.

An examination of the fœtus (which weighed six and a quarter pounds) showed that it was fully developed and only slightly macerated. The placenta corresponded with the full term of pregnancy, but was dry and bloodless.

The patient did well at first, but in a few days became septic, as the pouch behind the uterus did not drain perfectly. To correct this a counter-opening was established in the cul-de-sac (it ought to have been made at first), the good effect

of which was soon evident. After the continuance of a high temperature and an unusually rapid pulse for a fortnight, with an exhausting septic diarrhœa, convalescence was finally established, and the patient was able to be up a month after the operation, the sac having contracted to the capacity of a couple of ounces. She was discharged at her own request July 6th, as there remained only a small sinus at the lower angle of the abdominal wound. Two weeks later she was readmitted, as her mental condition was such that she could not be kept at home. It was evident that she was in a state of melancholia, with suicidal tendencies. Locally her condition was much improved; the remains of the sac were represented by an elongated induration as large as the thumb behind the uterus, the organ reduced in size and somewhat anteverted. There was no tenderness on pressure at any point. A probe passed into the fistula at the lower angle of the abdominal wound entered to the depth of three inches.

It was impossible to keep the patient in the hospital more than a week, as she refused to take nourishment and twice attempted suicide. At the end of that time she was transferred to an insane asylum, where she still remains.

Several interesting questions suggest themselves in connection with this case, which may be considered most conveniently under the heads of pathology, diagnosis, and treatment.

Pathology.—The condition found at the time of operation —the symmetrical disposition and perfect development of the sac, with the apparently normal condition of the uterus and adnexa—would at first sight seem to prove that the case could fairly be regarded as one of primary abdominal pregnancy; but the history clearly points to an original tubal pregnancy, with intra-peritoneal rupture.

That a careful post-mortem examination may fail to reveal in which tube the impregnated ovum originally developed before it was extruded is shown by the two cases reported by Sutugin. My own opinion is that the patient had in the latter part of June, 1891, an early abortion, and became impregnated in the right tube very soon after. When exam-

ined three months after, the uterus was so much enlarged as to resemble the normally pregnant organ. Two weeks later the tube ruptured—not into the broad ligament, but into the general cavity, as a tubal abortion after the second month has by Sutton been shown to be anatomically impossible, on account of the usual closure of the fimbriated extremity of the tube. The placenta, on being expelled into the peritoneal cavity gravitated to the bottom of Douglas's pouch, and at once became attached to the posterior surface of the uterus and right broad ligament. The accompanying hemorrhage was, fortunately, moderate, so that the patient did not show its effects. A localized peritonitis naturally followed, the resulting adhesions shutting off the fœtus, placenta, and blood-clot from the general cavity. The clot later became liquefied. In consequence of the long-standing irritation of this foreign body (and doubtless also from a low grade of peritonitis) a regular sac was formed, which enlarged symmetrically with the growth of the fœtus and the increased amount of exudation into the interior. Being fused with the uterus, it was naturally inferred that this organ was gradually enlarging as pregnancy advanced. The failure of the physician to detect either the fœtal heart-beat or movements is not strange, considering the abnormal position and relations of the fœtus.

When I saw the patient in April (at which time her confinement was expected) the fœtus was dead, and what she regarded as evidences of life were doubtless the peristaltic movements of the intestines around the sac. As no pulsation was felt behind the uterus, the placenta was probably already bloodless.

An unusual feature in the subsequent behavior of the case was the rapid accumulation of fluid soon after the death of the fœtus—a phenomenon contrary to that usually observed. It is interesting to note how little the patient's health was disturbed by her abnormal condition, and that there was an entire absence of symptoms pointing to other than normal gestation, from the time at which the tube ruptured until a month after the date of her expected confinement.

Diagnosis.—This has been thoroughly discussed in a recent paper by Sutugin,[1] who reports two cases similar to mine (but fatal) and refers to others. Contrary to the positive statement of Tarnier and Budin, in the case in which he made a correct diagnosis, Sutugin noted distinct intermittent contractions in the sac, which were afterward explained by the finding of muscular fibres in its wall. The absence of these in my case was regarded simply as strong negative evidence against the existence of intra-uterine pregnancy. These are, however, equally absent in cases of hydramnios with extreme flaccidity of the uterine wall.

Considerable stress has been laid upon the fact that in abdominal gestation the fœtal parts can be felt with unusual distinctness through the abdominal wall. This was by no means a prominent feature in my case. In fact, the fœtus was felt with far more startling clearness in a case of hydramnios in which I was called to perform cœliotomy, but delivered *per vias naturales.* My experience with these puzzling cases has been similar to that of our distinguished Fellow, Dr. Englemann, whose paper on "Abnormal Thinness of the Uterine Wall" was read at the last meeting of the Association. Tarnier and Budin attach no little importance to the absence of internal ballottement, associated with the presence of a soft, boggy mass behind the uterus, which may or may not pulsate. This was not clearly marked in my case, but still it was present, and I would not mistake it on another occasion, though the characteristic sensation imparted to the examining finger was obscured by the surrounding fluid. If the body of the uterus can be identified, of course intra-uterine gestation can be positively excluded; examination per rectum may throw a good deal of light upon the position of the organ. If there is even a reasonable doubt that its cavity may be empty, it should be thoroughly explored with a sound, especially if the pregnancy has reached term and the fœtus is alive.

[1] Zeitschrift für Geb. u. Gyn., Bd. xxiv, Heft 1.

It may be briefly stated, then, that in the case of a woman who presents such disturbances early in pregnancy as to justify the inference that it might have been originally tubal, and who develops a thin-walled gestation sac in which fœtal parts can be felt (whether the movements and heart-beat are detected or not), and when, on vaginal examination, we find an elastic retro-uterine tumor (especially if the enlarged corpus uteri can be mapped out), we should begin to suspect that the pregnancy is not intra-uterine. As the abdominal tumor enlarges, the thinness of the sac and the irregular increase in the contained fluid, with the presence of certain pressure-symptoms (pains in the back and abdomen, vesical and intestinal disturbances), will strengthen this suspicion and lead the attendant to insist upon a thorough examination under anæsthesia. If the diagnosis has not been made before, it certainly will be after the expected date of confinement has long passed without visible results, though this is now too late for the diagnosis to be of any benefit to the fœtus.

It is only fair to note that the absence of nearly all of the symptoms detailed in the case cited was what made the diagnosis so obscure.

Treatment.—It seems hardly necessary to dilate upon this point, as our modern surgical training naturally inclines us to adopt the only rational method of procedure—abdominal section. We do not need to review the disastrous results of vaginal incision to know this is an exceedingly dangerous and unscientific method of emptying the sac. It is, as Sutugin observes, tempting to evacuate its contents in this way, but, as he found to his cost, there is imminent danger of fatal hemorrhage from laceration of the placenta; and the surgeon proceeds blindly, not knowing the exact relations of the fœtus or sac. In a case of doubtful diagnosis I would prefer to make an explorative incision and find a pregnant uterus than to expose the patient to the danger of vaginal incision. It is highly important that the relation of the sac and its contents should be carefully studied. If the child has been long dead and the placenta is bloodless, it should, of course, be peeled off

at once. Nature usually indicates the best way of disposing of the sac, *i. e.*, to stitch it in the abdominal wound and drain it. There is no object in trying to enucleate it if it is perfectly isolated, as in my case. I believe that I made a mistake in not establishing a counter-opening in the vaginal fornix at the time of the operation. A certain amount of septic infection is to be expected; there is an unusually large cavity, containing many pockets, and we cannot err on the side of free drainage.

COMBINED SURGICAL AND ELECTRICAL TREATMENT OF THE BRAIN IN EPILEPSY.

With Report of Three Cases.

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Electricity has been made use of quite extensively in the treatment of epilepsy and allied brain diseases; but this treatment was never looked upon as anything but an interesting novelty. Having, like other surgeons, met with failure in benefiting epileptics by operative means alone, I decided to try galvanism anew, but by direct application to the brain, or at least to its diseased parts; of course, after trephining the skull. I may state at once that I have met with but moderate results in my experiments on three patients, still I have become convinced that electricity will, if studied more thoroughly, prove to be one of our most trustworthy agents in benefiting this large class of sufferers. I thought it proper to publish my experiences not so much in order to give advice in the method of procedure as to stimulate the interest of those who have more extensive material at their disposal.

But before proceeding to the report of my cases I must explain the reasons and conjectures which led me to select this new method. I will do it as briefly as possible, avoiding quotations as much as possible. Of course, it is mainly a theoretical and speculative discourse—a very undesirable feature of a medical paper, I confess—but there must be some

basis to work upon, and it is far better to have a weak one than none at all.

A great difficulty is encountered in the very beginning when we seek for a definition of epilepsy. The many recent discoveries in the anatomy and physiology of the brain, and the brilliant achievements of modern surgery, must not deceive us. As a whole the finer mechanism of the brain structures and organs is at present just as much of a mystery as ever—in health as well as in disease. I do not intend to go into an elaborate classification of all the different forms of epilepsy; all I am after here is to find a common characteristic for all possible varieties.

The division into genuine, Jacksonian, traumatic epilepsy, into minor and major disease, psychical equivalents, etc., shows what a variety of symptoms we are confronted with. In fact, neither the absence nor presence of consciousness, nor that of will-power, neither a certain motory nor a certain sensory or psychical disturbance is pathognomonic. It will be a more or less different picture in each case, because there is no brain-cell which may not be the seat of the disorder, and adding to it the millions of possible connections with other cells which may become secondarily affected, we see really no limit to the number of variations possible. Evidently the morbid phenomena will depend upon the functions of the involved brain-cells or rather brain-foci. There are hundreds and thousands of cases where the symptoms are so slight that they are hardly considered worthy of attention. They may manifest themselves as a passing drowsiness, a headache, a peculiar sensation, a slight aphasia, lasting only a second, a strange thought, a curious smell, etc., still they differ only in the locality of the affection, or more exactly speaking, in the character and in the number of the brain foci affected, whilst the pathological process may be identical with such as produce the most severe forms. Now, in my opinion, the salient point, the pathognomonic feature of epilepsy is the *habituality* with which a certain brain-focus acts abnormally, that is, independently of the normal controlling influences and pro-

cesses of life. It is always the very same focus which acts viciously in a given case of epilepsy. The disease, at least in the beginning, is not a general nervous disease, but affects a limited, definite cell-cluster of the brain. Therefore the symptoms in a given case must always be the same; they must repeat themselves in every attack. Seemingly this rule does not hold good in cases where several symptom-complexes alternate, so that, for example, there may be a minor attack at one time and a major attack at another. But evidently several foci are here out of order, and now the one and then the other will be set into morbid action. The minor ones will, however, be of the same character amongst themselves, and so also will be the major ones.

It seems to me that in this way a clear line is drawn between epilepsy and such diseases as resemble it. Take, for instance, infantile eclampsia. There may be such a close identity of symptomatic appearance that even the specialist will, when brought to a new case, hesitate to make a differential diagnosis at once. But eclampsia is an occasional disease, and if attacking the same subject will most likely look different every single time. One time the twitching will begin in the finger, the next time in the face; once the succession of symptoms will be one way, at another time just the reverse, and so on. The same is the case with uræmic eclampsia. Evidently here it is not an habitually disordered brain focus that is unduly irritated, but it is a general affection of the brain, of which some parts respond more briskly to a given irritant than all others do. From such considerations I would also classify such cases of hysteria and of hystero-epilepsy which present the same symptoms in every attack, under the head of true epilepsy. If the manifestations appear more irregular and erratic, I would rather be induced to look at the very centre of control itself for the involved focus than to seek for another morbid process with another name. We might say that in such cases the switchboard is habitually morbidly excited, that it is an epilepsy of the centre of control. Every general practitioner will agree

with me if I—with all due respect to high authority —call Gower's table of differentiation between epilepsy and hystero-epilepsy not a reliable and practical guide in diagnosis. In many a case it is almost worthless.

Granted now that the nature of epilepsy is a habitual disorder of one or more definite brain centres, the more important question arises: What are the causes of this disorder? Evidently there are three possibilities. 1. The brain centre itself is abnormally constructed; or 2. There exists outside of a sound focus a mechanism which acts on it exclusively as an abnormal irritant; or 3. Both these conditions are present—an abnormal centre and an abnormal extra-focal factor. Speaking in the usual medical phraseology, there is either an over-irritable brain centre, or an abnormal irritant to a sound centre, or an abnormal irritant and at the same time an abnormally irritable centre. There is no reason why certain brain-cells should not be faultily constructed from defective development—by heredity or otherwise. Just as the ear for harmony may by Nature be cruelly sensitive to discords in one individual and perfectly obtuse in another, so any other cerebral cell will, in different individuals, be differently sensitive and irritable. Is it to be wondered at, that the millions of human specimens show so much homogeneity in formation and function, considering that each of the millions and billions of cells has to perform a similar function in every individual? If, then, by some whim of evolution, it should happen that one or the other brain-cell or cell-cluster should develop below or above the average, it may lead to epileptic irritability either on provocation derived from without or emanating from a physiological product in an otherwise healthy system. In such instances the abnormality in the cell construction will be atomic and not amenable to detection.

Fortunately, though, another class allows us to make a pathological diagnosis by the gross appearance of the changes. They are ante-, intra-, or post-partum defects, lesions, or new formations of the most varying nature. Such changes may

render the functions of a certain cell-cluster itself abnormal, or the adjoining cells may suffer by contact or by other influences emanating from their primarily diseased neighbors. It is important to keep this latter mode in mind when studying cases of epilepsy, for evidently where there is a cyst, or a perfect atrophy, or any other total substitution by pathological tissue, no action, perverse or otherwise, can take place from this point. The adjoining or in other ways connected centres must be the ones which act viciously by being unduly influenced. By and by they will, of course, become permanently disturbed themselves, by being habitually irritated. Under all circumstances, then, in this class of cases, the epileptic action will be due to abnormalities of some brain centres.

Different will be the process when all the cells of the brain are of average health, but when there exists in the body somewhere a source of abnormal irritation which, from one reason or another, affects only *one* certain brain-focus and no other—if a depressed skull, a tumor, a bone spicula, or something else similarly direct can be detected—the irritating cause seems plain enough. If a cure follows the removal of this cause there can be no doubt that the brain-focus was and remained sound. More difficult to understand is the so-called reflex epilepsy. Of course, the signal given by a peripheric aura is by no means a proof of the peripheric origin of the trouble, because the action of a sensory brain centre will, according to physiological laws, make itself first perceptible at the peripheric terminus of its nerve circuit; still there are doubtless cases of reflex epilepsy in a centripetal direction. Cures after excision of cicatrices or after removal of adherent prepuces, after readjustment of malplaced uteri or ovaries, etc., admit of no other construction. Also, disturbances within the nerve tracks may be the causes of epilepsy, so that the removal of the diseased portion may lead to permanent cures.

But the cause of the abnormal irritation of a physiologically sound brain centre will often not be so visible or demonstrable. It will then be some substance, formed somewhere in the

body, which from some reason will act toxically on that centre. To be understood, I before presupposed that a viciously constructed brain-cluster will respond viciously on provocation of a normal constituent of the body fluids, whilst I now assume just the reverse: a normal brain centre but an abnormal irritating substance circulating in the body. There are, then, two points which require further explanation: 1. Are there such toxic substances found in the body? and 2 Why do they only affect a definite focus and no other?

The first point is settled, I suppose, by modern investigation, in the affirmative. Direct experiments prove that many of the physiological products of cell-katabolism are poisonous, and there can be no doubt that there are provisions made in the healthy body for annihilating, counteracting, and preventing them from doing harm. It would be superfluous to go over this field more exhaustively. I remind you, though, of the most striking instances, the myxœdema and the cachexia strumipriva when the function of the thyroid gland is defective; I remind you of the relation that diseases of the pancreas bear to diabetes; of diseased renal capsules to Addison's disease; of diseased ovaries to osteomalacia, and so on.

Life is the work of a socialistic action of all the cells of an individual. They all have to do their specific work, producing, eliminating, transforming, or whatever each one is destined to do, in order that it may contribute to the common good. Each, again, draws its supply out of the common current, according to its needs. To keep up the condition of health, each cell must do its work properly, and, consequently, each must find its proper nourishment. Suppose, now, that certain cells, or say a certain organ fails to do its duty, that it either delivers abnormal products or that it fails to eliminate certain obnoxious substances, then a disturbance in the economy is the consequence. Such a disturbance, when it involves an important part of the system and when it lasts for some time, will appear as a well-developed disease.

Now, as to epilepsy we may assume that in certain bodies

substances are formed by a defective action of some cells, which substances act only on certain definite brain foci by a selective affinity. It is, then, an idiosyncrasy of this brain focus for a certain substance. But I will say a few words on this point later on. To strengthen this position I may state that epilepsy has been observed as being caused by diseases of the kidneys and ovaries; that myxœdema is sometimes accompanied by it (Kocher); and that infectious diseases, like measles, scarlet fever, etc., have started the trouble. A German enthusiast has even claimed that he has detected a specific bacterium. A French authority, Marie, holds that the primary cause of idiopathic epilepsy is always the result of a specific infectious disease, which appears in the form of an acute cerebral inflammation, an opinion which seems to me to reach too far. Dr. A. B. Griffith claims to have obtained a specific leucomaine from the urine of epileptics. Many more proofs of more or less reliability may be found in modern literature.

I think that this theory of a toxic process, as the cause of the abnormal irritation of a certain brain-focus—healthy or not—is by far a better explanation than the theory of a stored-up electric force in a brain centre, which theory would have again to be explained by a number of other presumptions. The difficulty lies mostly in the answer to the very justifiable question: Why should only this definite centre, and no other part of the brain, be so influenced by the supposed toxine?

The principle of affinity between certain tissues or cells and certain substances is conceded by medical observation and governs a good deal of medical interference. Nobody doubts that digitalis acts specifically upon the heart; belladonna upon the iris; ergot on the womb, etc. Likewise, pathology is full of evidences of the existence of such a specific affinity between certain obnoxious substances and definite tissues. Take, for instance, the constant implication of the conjunctiva in measles, of the palate in scarlet fever, of the peculiar form of delirium in alcoholism, of such in typhoid fever, etc. All these facts go to show that there are specific relations existing in each

disease. But even substances which are generally considered wholesome, which are considered regular food, act sometimes as specific toxins to definite body tissues, a condition which we call idiosyncrasy. In fact, nothing teaches so well the truth of the toxin theory as the interesting disease, urticaria; and, again, that form of it which is called acute circumscribed œdema, wherein only a certain part of the subcutaneous cellular tissue becomes involved. I had the opportunity to observe two such cases which are interesting enough to be briefly described. One case is a middle-aged gentleman who, from time to time, consulted me for a strange, itchy swelling, confined to the right side of his upper lip. After repeated observation it became evident that these signs invariably followed the partaking of oysters or sea-crabs the night previous.

The other case occurred in a gentleman with old syphilitic traces and who had the swelling in the same location. It disappeared on anti-syphilitic treatment, to return twice, as soon as the treatment was discontinued. If we apply such specific actions of certain toxines to limited portions of the brain, we have, without too great a stretch of the imagination, a probable cause of an epileptic attack. But nothing seems to me to give more indisputable evidence of the existence of such a specific relation than the phenomena connected with Koch's tuberculin. For whatever we may think of its efficacy as a curative agent, there can be no doubt that it has a specific action upon definite tissues. At the same time this wonderful fluid shows how extremely small quantities may suffice to affect extensive tissues even up to perfect destruction.

The explanation of such a selective relation would require a knowledge of the profoundest secrets of biology; yet nothing but theories are at our disposal. Of the four or five lately offered, none seems to me as rational as the beautiful one of my friend Dr. J. W. McLaughlin, of Austin, Texas—a theory which has the great drawback, though, of coming from a modest Texas practitioner. It has been published in the *Medical Record* of New York, and in an enlarged form in

the *Texas Sanitarian*.[1] This theory is based upon the present views of motion in matter, according to which all physical, chemical, biological, in short, all kind of perceptible phenomena are products of atomic and molecular vibrations which are conducted by and reproduced in the surrounding ether. The different manifestations are dependent upon the differences in the nature of the waves, which are specific with the different cells. But the waves of different molecules will interfere with each other, and thus the dynamic energy of a molecule, and finally of a cell, will be modified by interfering waves of another molecule, organic or inorganic. Now it wholly depends upon the relationship between the waves of the interfering and the interfered atom or molecule whether a certain modification, appearing as modified energy, will take place or not. Like acoustic waves may and may not interfere with each other, as one tone may increase the strength of another at one time, whilst it may annihilate it at another; in short, as there is in acoustics a plain selective interference amongst tones, so it will be in every other form of energy. Now, applying such a theory to the biological processes of the human cell, we may say that the cells of the body must have waves that are not disturbed over and below a certain endurable average by each other or by the usual physiological constituents of the food and systemic juices. A kind of equilibrium is maintained which we call health. But as soon as the molecular waves of a certain cell or cell-cluster are so altered as to allow undue interference by a certain otherwise normal cell or substance; or as soon as such abnormal substances—poisons, toxins—which exert an undue interference on healthy cells, enter the circulation, phenomena of disease, of disturbed energy, in short, of abnormal wave-action will appear. As there must be a certain relation between the waves, it is evident that not all brain-cells will respond to the wave interference of a certain substance, but only such as are

[1] It is published now in book form under the following title: "Fermentation, Infection, and Immunity."

accidentally in effective relation to it. But it is impossible to present an intelligent outline of this theory in the brief space at my disposal.

The mechanism of an epileptic fit would, then, be as follows: There is either a brain centre whose atomic action is set into undue vibration by a physiological product of the usual metabolic processes, or it is an abnormal product which interferes unduly with the atomic waves of a sound cell-cluster; or, thirdly, both the brain centre and the body products are abnormal and just in such an unhappy relation to each other as to produce the wave-action necessary for a fit.

Unfortunately, though, a new difficulty now arises. If such a relation between the two factors were the essential cause of epilepsy, it still remains unaccountable, why the faulty action should not be permanent, being the consequence of permanent conditions. There is obviously a third factor, which is usually called the incentive cause, and which demands a satisfactory explanation to make this theory hold good. If a strong occasional psychical or physical insult only would set an irritable focus into undue vibration, our theory would look plausible; also, if a more or less periodical function, like menstruation, should always be the incentive, we could understand the occasionality of the attack. In cases of syphilitic origin or of metallic poisoning—as in lead-epilepsy and other similar forms—it would be possible that the organic or inorganic virus remained deposited in some lymph gland, and that it might by some accident be washed into the blood or lymph current, and in that way find a chance to play on the involved brain centre. But when the patient, free from all such troubles, is attacked in one instance daily, in another only once in a month or in a year, without any perceptible provocation, say at night, then some new agent must certainly have been added to the mechanism. A third factor must exist for operating the supposed toxin, and this third factor can hardly be expected to arise from anywhere else than the outside of the body. It must enter the system by some accident—just prior to the inauguration of

the attack—exactly as the toxic substance is taken into the system, always anew, in every attack of urticaria. Such a substance may enter either by the respiratory tract, or, perhaps, hypo- or endermically; but most likely it will be a part of the food, and it is not at all necessary that it should be a poison or even an unwholesome substance in its ordinary acceptation. The mechanism by which it may interfere with the brain-focus is manifold. It may irritate, as our present medical jargon expresses it, the nerve terminals in the stomach or bowels mechanically, and set up a reflex action in the brain-focus (a prototype of such a process is the infantile eclampsia, which, as before stated, differs from epilepsy in not being habitual); or certain constituents of the food are imperfectly oxidized within the alimentary canal; or they may become fermented or putrefied, perhaps by the presence of bacteria, and thus toxic products are formed and absorbed into the system. Under such conditions they may act directly upon a favorably disposed brain-focus, or they may, in conjunction and with the aid of other viciously acting organs elsewhere, develop into the proper kind of toxin; or, finally, some faultily-acting organ may fail to eliminate them or to protect the brain-focus against them. Interesting researches have been made by Herter and Smith, of New York, in this direction. The idea that meat food had much to do with epilepsy is a very old one, but from the literature at my disposal, I cannot find any positive proof of the correctness of such an assumption.

Finally, there is yet another interesting explanation possible: A substance may be excreted into the stomach or bowels, and there, by the aid of the physiological juices, become reabsorbable as a toxin. This mechanism is experimentally proven by Alt's well-known experiments with morphine, which, after being injected hypodermatically, was found after a short time in the stomach. Reabsorption and the usual vomiting was prevented by removing it from the stomach by means of the pump.[1] Similar results follow the

[1] Alt has proven the same process in the elimination of other poisonous substances like snake poison, and even cholera toxins.

washing out of the stomach in peritonitis and other septic diseases. The surprisingly prompt, though not always long lasting, relief obtained in this way, is in my opinion not due to the relief the stomach gets by being freed from its burdensome pressure, but results from the removal of poisonous substances which would otherwise be reabsorbed. That there is such a mechanism is proven by some very ingenious experiments by Bouveret and Devic, who found that certain toxins produce tetanic convulsions when brought into the stomach only when there is a surplus of free hydrochloric acid present.

I have purposely dwelt on this point a little longer because I believe that certain therapeutical measures may be based on this fact. To sum up, then, I contend that the incentive cause, which provokes the epileptic attack, is an accidental addition from without, which either acts as a toxin to the irritable brain-focus in a direct way, or with the aid of an already existing abnormal constituent of the body fluids or of a faultily functionating organ.

Taking up now the most important part of my paper—the treatment of epilepsy—it is plain from what has been said, that there are two ways to approach the trouble. Either the cause of the irritation to the brain-focus is to be removed or counteracted; or, secondly, the brain-focus itself is to be made resistant to the irritant—it is to be rendered immune.

Wherever a direct palpable cause can be made out, it is, of course, proper to remove it either within or without the brain. But it is a fact that, even after seemingly successful operative undertakings, the epilepsy is likely to recur in the course of time. Considering that the visibly changed tissues, as stated before, are not always the seat of the epileptic process, but that they have rather caused the neighboring brain tissues to become disturbed, and that these latter have remained untouched in the operation; considering further, that cells which have once acquired a vicious habit may retain it after the removal of the primary cause, and considering, finally, that cicatricial tissue or adhesions will easily form after cutting operations, and act as new irritants, then it is obvious that

in most cases the treatment must not end with the surgical interference.

If the attacks should be caused by direct irritation of the nerve terminations in the stomach, and a consequential reflex, they may be prevented by avoidance of coarse food and unwholesome habits, like swallowing unmasticated or tough food, etc. Perhaps the irritability may be diminished by sedatives. A very bold way to break the reflex circuit is to sever the sympathetic nerve between the stomach and the brain. This has been done by Jaksch, who in two cases made section of the inferior cervical ganglia, and he reports cures that have already lasted one year. But it is not this point that interests me here. Neither do I wish to go into the diet question too extensively. I have already stated that meat has been accused of being instrumental in producing epilepsy. This may be so; perhaps meat supplies the chief ingredient of the toxins, which are mostly toxalbumins, but meat is certainly not an absolute requirement for their formation. Up to the present time but little has been learned about it, and supposing that, in every given case, a different idiosyncrasy of the brain-focus may exist, it is hard to imagine that a satisfactory account of the myriads of possibilities of chemical and physical combinations will be found.

Of greater value is the prevention of the formation of abnormal fermentation and of putrefaction of the contents of the stomach and bowels. Anti-fermentatives, therefore, may act well, and in fact do so in some cases. Going over the list of new remedies collected in Sajous's *Annual*, there are some which may act in this way, as borax, phosphoric acid, etc. Induced by the miraculous effect of carbolic acid in chlorosis, which I also consider to be a toxic condition, and following Nothnagel's advice therein, I have also tried it in five cases of epilepsy. The drug may act directly as an anti-fermentative to the contents of the stomach and bowels, though it hardly looks possible that the small drops in use should be sufficient for such a purpose. Another explanation is that carbolic acid annihilates certain toxins which accumulate in small quanti-

ties in the stomach, being eliminated there, which toxins are the very evil-doers. But perhaps it takes effect by contact with the brain-cells themselves after having formed salty compounds in the blood. But, whatever the modus operandi is, there is no question in my mind that carbolic acid is a palliative remedy for epilepsy, better than bromides wherever disturbed digestion is present. The dose is from three to seven drops, three times daily, given in water or syrup. In my cases there was no discoloration of urine, and no patient has in any way complained of his medicine. In all of them, including a six-year old child (dose one drop), the quantity and quality of the attacks changed for the better, but most striking was the improvement of the general mental behavior.

The next way to exert a therapeutic influence upon epilepsy would be to counteract or to decompose the toxins formed within the system as a product of cell-metabolism. Unfortunately, this is altogether a field of speculation. It will take many years of hard experimental work to elucidate even a few of these complex processes. The therapeutical selection will be, therefore, nearly altogether arbitrary and empirical. Whether or not the remedies now in use act in such a way or not, cannot now be decided. I, myself, do not believe that they do. They take effect, I think, upon the brain-focus itself.

The little promise offered by the direct approach to the vicious mechanism by way of acting upon the toxin itself directs our mind to the second way—that is, to the improvement of the condition of the disturbed focus itself, or, if this should happen to be sound, to render it able to withstand the abnormal irritation. In the first place the indication is to make the nerve-cell proof against the toxins which act on it as a specific poison. The explanation of immunization is yet but theoretical. But again there is, in my opinion, none better than that of McLaughlin.

On the basis of the before-mentioned interference of one wave upon another, we may suppose that certain substances are able to permanently modify the specific wave-action of a cell in such a manner as to make it proof thereafter against

the abnormal influences theretofore exerted upon it. The susceptibility will be changed into immunity by artificially changing the molecular or atomic wave. The immunizing substance must have the power through its specific waves to disrupt the atoms and to force them into new combinations with new waves. McLaughlin's theory beautifully shows how such a premise could be imagined, and how such a theory would explain all the phenomena brought out by modern investigation.

But while I concede that the process and nature of giving immunity may be disputed, the fact itself is, in the light of modern researches and experiments, hardly questionable. The daily experience of our science offers hundreds of examples of the specific relation of toxic substances to certain tissues, so that these latter become permanently changed for the better or the worse by the first. This is, of course, the fundamental process of immunity. To make myself understood, I will cite a case that I came across in literature: a rhinoscleroma disappeared during an attack of typhoid fever. Evidently the toxins produced by the latter acted as an annihilator of the cells of the new-growth. A case just the reverse, where an infection brought on a permanently strange condition of certain tissues, is that of a gentleman in my practice, who, after having once been stung by an insect, was never able to eat meat of a quadruped without being thrown into an exquisite attack of urticaria. Here the toxin caused a permanent change in certain cells; it brought on a kind of reversed immunity—a susceptibility. You have all, perhaps, read the report from the Chicago Pasteur Institute, that an epileptic boy was cured of his trouble by inoculation against hydrophobia. This strange experience led to further experiments in this direction, but I do not know of any reports of them, except of one case treated by Giovanni, which is said to have recovered from maniacal attacks and epilepsy under the same treatment. There are cases on record directly proving the correctness of our position, where epilepsy disappeared through the aid of intercurrent infectious diseases like typhoid fever,

etc. The point I intend to make is that it would not be absurd to search for something that would so alter a brain-focus as to make it proof against certain toxins. If there are substances which are able to produce epilepsy, such as oil of tansy, ergot, absinthe, etc., then one could reasonably suppose that possibly some substances could be found as well which would change the atomic action of a certain cell just in the reverse direction. Up to this time, though, no such agent has been found. The salts of the bromides are, as has been proven, deposited in the brain-substance, and whilst they no doubt exert a soothing and relieving effect upon the diseased brain-cells, or, as it is called, lower their excitability, this seems only to be the case as long as the bromides remain in contact with the brain. It is, therefore, no genuine immunity. The same is the case with other remedies which exert a palliative influence on epilepsy, as antipyrine, chloral-amide, ethyl-bromide, or the recently recommended piperazine. Very interesting is the experience of Prof. Babes, who insists that nerve-substance injected hypodermatically had an excellent effect upon epilepsy. Who would not in connection with this recall Brown-Séquard's testicle juice! Indeed, the many surprises bacteriologists find in the study of the blood-serum as an immunizing substance against diseases; the difference of this action in different persons; the remarkable power of the thymus, and other strange facts, should make us feel hopeful of the final detection of a reliable immunisator for the diseased brain-cell as well as for any other cell of the human system. However, it would seem that the atomic wave disturbances of the brain-cells must be so varied that the agents which could cause this immunity would also have to be innumerable in order to exactly meet the requirements of breaking up all of the former. But I do not wish to detain you longer with such merely speculative matters.

The poor prospect which medicamental substances offer to alter the brain-cell so as to make it bomb-proof against the influences of abnormally irritative forces led me to look to electricity as a more powerful and a more direct means for

meeting these indications, a means which can, furthermore, be modified at will in strength, direction and local influence. I thought that the electrical wave might be the right one to interfere in the desired way with the nerve-cell atoms, if only the proper mode and proper dosage, etc., could be found.

The idea that a faulty wave-action should be more or less mechanically broken up and a new mode of vibration caused seems to me not to be too far fetched. To me it appears to be the explanation of the electrical influence in general. It is founded upon the same basis that Boudet's and Charcot's Vibration therapy is, or Granville's percussion treatment. To use, perhaps, a very crude comparison, the idea is to bring on a change similar to that which takes place when cream is converted into buttermilk and butter. The aim is to cause the atoms or molecules to arrange themselves differently.

I selected the galvanic current for several reasons. The experience of some experimenters in brain physiology teaches that the faradic current when often applied to the brains of animals made them epileptic; though used occasionally on man in operations on the brain (Horsley, Keen, and others) as a means to better localize the diseased focus, has not proved detrimental. Still, it seemed to me safer to try the galvanic current, which is not so irritating. Besides it can be applied in a much greater quantity. Finally, trustworthy observers (like Prof. M. Meyer of Berlin) insist upon the dissolving action of this current upon cicatricial tissue. Thus, if there should be fibrous or gliomatous degeneration, the possibility of destroying the abnormal tissues by a kind of electrolysis would be met. Still, I would not insist upon the use of the galvanic current exclusively; on the contrary, I would not be surprised if the faradic current, under certain circumstances and used with great care, would not turn out as the more effective. Nothing but experience will settle this question.

To select the proper strength of the current is likewise at present but a matter of chance. If the theory on which I base my views be correct, then almost in every case a differ-

ent strength will have to be employed to meet the atomic wave in the right way. But, perhaps, an average strength may be found. I helped myself out on this point by slowly increasing and decreasing the current, so as to multiply the chances. As an average 5–12 milliampères were given and well tolerated. It seems that after a time the brain becomes less tolerant and that the strength has to be gradually reduced. My first case in the beginning stood as much as 30 milliampères, whilst at the end 5–8 were all he could tolerate with comfort. There occurred to me another objection, which demanded an explanation.

On the theory already advanced, one single well-adapted shock of the current ought to be sufficient to permanently alter the waves of a certain cell. But, from all experience, a long lasting, repeated and again repeated electrization is necessary in electric cures. I accordingly sought for excuses, so to speak, to meet these apparent inconsistencies. In the first place, we may not always succeed at once in turning the waves so as to make them remain in their new direction. They may rebound like an elastic body and only remain in their new condition after a great many similar distortions. A second explanation is, that each disturbed focus consists of a number of single cells, and that each having its own specific action will not all be equally reached by the same current; and that it will therefore take time to hit them one after another, like a target which we wish entirely to perforate with bullets, especially since we may consider that there will be different layers of cells situated one above the other. But it may be surmised that the waves of some cells never will be met by the current, but that as soon as the neighboring cells change their action, the former will be influenced by them; or that a small minority of unchanged cells will not be in the way of the improvement of the whole cluster.

In order to apply the galvanic current directly to the brain, trepanation must, of course, have been previously performed. The advantage of such a method over the old way of sending the current through the scalp and cranium is evi-

dent. A comparative test with the same electro-motive force gave 17 milliampères when the electrodes were held to the dura mater on two points, whilst only one milliampère could be gotten when they were applied to the skull close to the same openings. The direct current can be felt all through the body, whilst hardly any general sensation is produced in the second instance. In a test comparison between the brain-substance of a calf and the bones of the skull of the same animal it was shown that the bones of the skull offer twenty times more resistance than the brain. The current would therefore have to be at least 100 or more milliampères when applied externally to correspond with 10 milliampères, when applied to the brain, which latter can be done without any hesitancy; while the former would be, of course, unbearable. The trephining itself, is as generally conceded nowadays, an almost harmless procedure, and considering that this operation in itself not infrequently gives relief, I think that nobody will find my design too risky and hazardous.

But at what place and on how many places should the skull be opened? It seems but reasonable to use the trephine over the middle of each motory zone or over such regions as, according to the rules of localization, are considered the diseased foci. From these points a larger circle of the cortex could then be influenced. Three or four small openings could be established without increasing the danger from the operation or from a consequent defect of closure. If there be only one opening, the other electrode may be applied somewhere on the scalp or on some distant point of the body in such a direction as to force the current through the suspected portion of the cortex. It looks very reasonable to direct the current from the point where the aura starts to the corresponding brain-focus; but in fact it only looks so. The current takes the most direct line, and does not care for a particular nerve-track as long as the medium through which it travels is a homogeneous conductor. Still, under certain circumstances, the line of the current may exactly coincide with the anatomical path. In one of my cases the application of one of the electrodes to

the wrist, which was the starting point, and which always felt weak and dull, had a decided instantaneous as well as permanent effect upon it. As the motor, sensory, and psychical cells and centres are distributed through the whole gray mass of the brain, we will, under some circumstances, have to reach the lower aspect of the brain or the basal ganglia, the nuclei of the cranial nerves, etc., and consequently under these circumstances the route from the upper surface across or through the brain to some point in the body will have to be taken.

As a matter of course, the effect of the current will be greatest at the point of application and in the direct line between the electrodes. In a vessel of the capacity of a skull, filled with water, the electrodes being 5″ apart from each other, 50 milliampères were produced within this line, but even at 6″ from it, the current from the same same source was reduced to 2 milliampères, showing how rapidly the effect diminishes with the distance from the electrodes. It is very important, therefore, to get as much of the diseased area between the electrodes as possible, which can be done in two ways. The electrodes are either made very large, or the objective area has to be traversed in several directions. I think that it would be the better method to have long linear openings (as in linear craniectomy), along Rolando's fissure or any other location where the trouble seems to be situated, and to have corresponding electrodes. Perhaps three or more small trephine holes, placed in a line over the place of selection, will give the least discomfort afterward. Next I would like to see the current applied for a decidedly longer time than the usual seven to fifteen minutes. The occasional excellent results from galvanic chains and similar popular appliances seem to teach that there may be something in an almost constant application. Perhaps the electrodes could be so fixed as to remain in place for hours, while the patient might engage in some work not requiring much motion.

In galvanizing the brain there is one feature which is of material aid to us and which ought to be taken into consideration. The organ is suspended in a vessel filled with a fluid,

which, containing about 0.8–0.9 per cent of chloride of sodium, is a good conductor. Whether the subarachnoidal spaces communicate with the layer of fluid between the arachnoid and the dura, or not, makes hardly any difference. For our purpose it is a continuous fluid surrounding the whole brain and entering into all the ventricles, because the thin membranous partitions offer no resistance to the current. Thus, every portion of the surface and of the lining of the ventricles is subjected more or less to the influence of every current sent through the brain.

Now, as to the pain caused by the current, applied directly to the dura. Some statements that I found in the literature on this subject made me apprehensive lest the whole plan would fail on account of the sensitiveness of this membrane. But fortunately it turned out to be only a bugbear. Of course, a strong current applied to the dura would burn it up. But *moderate currents do no visible damage and are not painful.* I used a pellet of cotton, moistened with salt water, between the dura and the electrode, and did not experience the slightest trouble.

The next point was to discover how to isolate the current from the scalp and bone. For this purpose I had some tubes made of hard rubber to fit the trephine holes. A cotton pellet was pressed into one end, and a metal rod through the other, representing the electrode, was brought in contact with it. In this way all of the current had to go through the dura, which it would have done anyhow, since it is a better conductor than the bone and scalp; but the pains to bones and scalp were avoided.

Some trouble is experienced on account of the scalp wound healing too rapidly. I made the old crucial incision because it permitted me better to plug the wound, and thus keep it open. When the skin encroaches too much on the opening it has to be lifted up from time to time. Of course, after a while a layer of granulations covers the dura, but this is rather a protection against sepsis, whilst it does not change the resistance to the current.

It is superfluous to state that the utmost asepsis must be observed throughout. It is unnecessary, I think, to shave the whole head after a week. A good-sized area around the holes must be kept closely cut and washed daily with benzine, which removes the dirt and secretions better than anything else. The plug I used was made of iodoform gauze. A cap may be worn over the head. Less disfiguring is a cover of some black material, held firmly by an elastic band over the openings.

In regard to the selection of the poles I cannot state anything definite. It will require a very large statistical comparison to reach reliable conclusions. Perhaps the rule to use the positive pole as a solvent and the negative as an alterative will be of some avail. Perhaps, also, Prof. Morton's theory, as set forth in his interesting article in the *Medical Record*, will serve as a guide. We can hardly expect any rule to work in all cases if we accept McLaughlin's theory. We must simply try one direction of the current after the other in every case. There is no plus and no minus according to this theory. Still, some average *modus operandi* may be found.

It will be of little importance what kind of a battery is used. But it is absolutely necessary that a good rheostat and a good milliampèremeter should be attached to it. It would be criminal to do the work without such safeguards. One mistake may do irreparable harm. The strength of the current must be well known and the operator must have full control over it.

Before proceeding to give the histories of my cases I deem it necessary, in order not to appear as a heedless enthusiast, to explain my position fully. I am perfectly satisfied that electricity in some form, and applied directly to the brain, will yield excellent results in the cure of certain cases of epilepsy, and especially in the most intractable form, in the so-called genuine epilepsy. But I am equally well satisfied that it will take much work and study to gain the rules for its effective application. *I can claim for my cases only one unquestionable result, and that is a surprising improvement of the general mental and physical condition.* It was plainly and beyond

any doubt obtained even in my third case, where a large portion of the brain was in a decayed condition. There exists no doubt in my mind on this question.

In regard to the direct influence upon the epileptic attack, I may say that the soothing influence was pretty evident; its existence was felt by all three patients. They were all eager to be regalvanized, because they all felt as if their spells were milder. The frequency of the attacks seemed to be temporarily favorably influenced; I know that mistakes—for instance, too sudden variations—often spoiled the effect. Eventually, though, the attacks returned with their accustomed frequency. But I firmly believe that a different application of the current will lead to better results. But just as certainly as I expect good from the galvanic current, so sure am I that success can only be had after a thorough knowledge of the science of modern neurology and modern surgery. He who expects to be successful must be able to make a correct diagnosis or must have the assistance of a reliable neurologist, so as not to lose time and the confidence of the patient by unnecessary delay of surgical relief in tumors and abscesses, or of specific treatment in syphilitic cases, and so on. In short, one must know when the treatment is indicated; he will then avoid such mishaps as I had in my third case, though this case was subjected to the galvanic treatment only to give him the benefit of the doubt and to give me one more case to experiment on. The treatment seems to me to be indicated in genuine epilepsy; in acquired forms, whether there is no evidence of a lesion; in short, in such cases as are not expected to offer detectable changes if operated on; and in surgical cases, after proper interference, as an after treatment. Here the trephine hole may be used for the subsequent galvanic application. Further experiments ought to be made in large institutions, with ample material and ample help. If I shall succeed in instigating such researches I will feel fully repaid for my private work, which was conducted for nearly a year.

CASE I. *A case of so-called idiopathic epilepsy.*—H. N., aged thirty-four years, a sturdy German, of clean family history, had his first attack eight years ago, from fright. In the beginning, twice a week; afterward, and at the time he applied for treatment, once a week, usually early in the morning and for several consecutive times on the same day of the week. He then gets blue in the face, his eyes look wild, his head and neck are pulled to the right, and general convulsions set in, after which he falls to the ground. No aura. He constantly feels a general dulness through the brain, which entirely prevents him from working. Feels flighty and insecure. His mental capacity is dulled. Has taken medicine for years, but, excepting at short intervals, he has never been free from attacks for over a week at a time.

Operation on February 20, 1892. Present, Drs. Wilkinson, Smith (U. S. M. H. S.), and Singer. Crucial incisions immediately over a deep scar on each side over about the middle of Rolando's fissure. There were so many scars that those best situated could be selected. The internal table of the left button appeared somewhat more injected than normal. Two days afterward, this being the favorite day, he had a slight spell; another of usual severity two days later. Up to this time nothing else was done, in order not to be misled by the common experience of improvement after trepanations. February 26th, six days after the operation, in the presence of Drs. Wilkinson and Singer, the patient was for the first time treated by the current in my office. He easily stood 30 milliampères, the electrodes being applied to the two trephine holes. He felt the current through the whole body, especially in the lower extremities and in the stomach. To our great amazement he could stand even 70 milliampères, which was tried, though only for a second. After a five minutes' seance he felt very well, and went back to the hospital, about a mile distant. February 27th, the next day, the application was renewed, but he could only stand 17 milliampères with ease. The pulse was watched before and during the operation, but no difference was to be observed. For the sake of comparison, the current from double the number of cells was applied close to the holes in the cranium, but gave only 1 milliampère. The applications were repeated daily.

March 1. Five days after the commencement of treatment, he had a spell of the usual severity. Galvanism of 12 to 17 mil-

liampères was then applied for fully 10 minutes on the following days.

6*th*. He had a very light spell, no unconsciousness. The same next day.

10*th* and 11*th*. The same. The attacks have entirely changed; they are short, with but little convulsive movements, and when over, leave him tolerably strong and well enough to attend to light kitchen work, a thing he never could do before after his previous attacks, when he felt perfectly exhausted for a whole day. For the next nineteen days applications were made with reversed currents, five minutes each way. He had slight dizzy sensations occasionally, but never a developed attack. His general mental condition showed the most striking improvement. His sulky, drowsy feelings have left him; he feels perfectly well and likes to work. The strength of the current is gradually increased and decreased during the seance.

30*th*. By mistake an overly strong shock was given.

31*st*. He had a slight attack, but again lost consciousness.

April 1. The same. From then on only 6 to 7 milliampères were used for fifteen minutes. He was not able to stand 9 milliampères. The patient remained well until the 13th, when a mild infection of the wound, with swelling of the lymphatics in the neck, occurred. No treatment. He did not have another spell until the 22d, when he had a slight attack without loss of consciousness. Galvanic treatment resumed on the 26th. He had a slight attack on the 29th. After an interval of nearly a month he had another, but very slight, attack on May 21st. He now gets bromides and no galvanism. He had a very slight spell on June 7th, but a severer one of the old type on June 23d. The holes which have closed are reopened on July 2d, and treatment again resumed. One electrode was placed on the dura and a large sponge electrode carried around the skull. He does not feel as well as before. The old way, with an electrode in each opening, is repeated on July 12th and continued for two weeks. He now feels so well that treatment is discontinued. He kept free from all attacks, and was seemingly well until the middle of August, when he got one of his old spells, followed by another a week afterward. Galvanism was discontinued, and 3 drops of carbolic acid given three times daily, gradually increased to 5 drops. The patient feels greatly improved in his general health.

Since then he had one slight attack after an interval of three weeks, another four weeks later, and a third two weeks after this, all light, and leaving him in good general condition. He is working now steadily, looks healthy, feels perfectly well, and is fully satisfied with his improvement. Up to date, November 17th, he had no further attack. He continues, though, the carbolic acid, 5 drops three times a day.

Case II.—S. B., American, aged eighteen years, born without instrumental help; no nervous diseases in the family. Healthy up to his sixth year. He then fell upon a nail, which was driven into his skull in the middle line, 5″ over the glabella. Since then headache. Five to six months after this he had "brain fever," without any particular cause. It lasted twenty-five days, during which time he was mostly unconscious, screaming and tossing about, but without convulsions. After he got well of the fever he constantly complained of headache.

In September, 1890, that is when he was sixteen years old, he was suddenly seized with convulsions, unconsciousness, and frothing from the mouth, the mouth being drawn to the left. The attacks were then very frequent, as many as two and three a day. He had occasionnal free intervals of from two to three weeks—once no attack for four months. He took medicine all the time. His left arm, and especially the wrist, felt numb and always weak, so that he could not make full use of it. Lately he got worse, and, on the slightest provocation, excessive convulsions, beginning in the left wrist, will set in. He feels drowsy and unable to do work. An attack witnessed by myself was of the Jacksonian type, beginning with twitching of the left hand, followed by strong left-sided convulsions, which then ended in a general tetanic condition with unconsciousness. On examination, his face showed a very decided dull epileptic expression. A scar was found in the middle line, 5″ from the glabella, with a fine fistulous opening; also another scar over the upper third of the right fissure of Rolando. The region around these spots is somewhat painful on percussion. The knee-jerk is exaggerated on the left side. Eyes, no choked disks, but abnormally distended retinal vessels. (Dr. Hall.)

Diagnosis. Most likely a benign tumor, perhaps a cyst in the right motor zone, judging from the permanency of the wrist

trouble, the knee phenomena, and the condition of the retina. Nevertheless, I concluded to give galvanism a trial, it having been agreed that should it fail and the patient get worse the dura should be opened and the brain examined.

March 12th (Drs. Smith, Wilkinson, and Singer assisting) the operation was performed at St. Mary's Infirmary. A small trephine was applied over the scar in the middle line, and from here enough bone removed with the plyers to expose a small cavity between the bone and dura which was filled with coagulated pus. The dura looked healthy, and pulsated. Another 1″ trephine hole is made over the scar of the right motory zone. The button is twice as thick in its posterior half as in the other, and some more thickened bone is removed with the plyers in this direction, leaving an opening of nearly 2″ anterior-posterior diameter. The dura does not pulsate, but does not bulge out. It looks healthy. Simple iodoform gauze packing of both openings.

March 18. Sixth day after operation; the dressings were removed. Patient feels very well; pulsation in both openings.

19*th*. Had an attack of usual severity.

21*st*. Between 12 and 15 milliampères were applied for five minutes, through both openings, in each direction. The patient describes the sensations as very pleasant. The stomach feels strange. Applications continued daily.

24*th*. An attack. No treatment for several days, during which time he had several mild attacks.

April 6. Galvanism was resumed. Sometimes he would stand only 6 milliampères, at others 20 milliampères. Always felt better immediately after the galvanism. The left arm gets stronger. Keeps free from attacks until the 19th, when he has a severe one. After this the current is applied, between the brain on the right side and the wrist on the left, in both directions. Does not stand more than 3 to 5 milliampères. Complains of impairment of sight. Ophthalmoscopic examination by Dr. Hall shows the retinal vessels to be very much distended, but no choked disks. No inflammatory changes. No hemianopsia. Same as at first examination.

May 4. Slight, as usual. Daily applications between the right hemisphere and left wrist in both directions are continued.

During the application he has the sensation of a swelling in the nose.

12*th.* Light spell; consciousness retained. Light spells next and following days.

24*th.* Severe spell. Gets bromides and no galvanism. No more spells up to June 7th. Bromides discontinued and sponge electrodes applied to the right trephine hole, which is covered by cicatricial tissue and scalp all around. Feels better. Left arm and wrist feel perfectly well. His mind is clear, and that old dull, drowsy feeling gone.

June 11. A light attack. After this some more galvanism, and then the applications were discontinued. Three weeks later he reports himself in splendid condition at my office, but gets a very severe attack right before my eyes. They return oftener now, but his left arm and general mental condition continue greatly improved.

August 10. Begins taking 3 drops of carbolic acid three times daily, and does well up to the 24th, when he has another light attack. Another on September 21st. Two more light spells two weeks apart from each other. His left arm and wrist feel perfectly natural; his mental condition perfectly satisfactory; looks well; continues the carbolic acid. Last seen November 10th.[1]

Case III. (Sent to me by Dr. Noble, of Barclay.)—C. R., American, aged seventeen years, without family history of interest. At eleven years of age, in 1886, he sustained a severe fall from a horse, remaining insensible for about twenty-five minutes. He complained a few days. Shortly afterward he was struck on the head with a heavy stone—does not remember on what side. Again became unconscious. In September following he had his first attack, which was very light. He tried to speak, but could not. No convulsions. About every three weeks for three years he had spells of aphasia, accompanied with grasping

[1] Case was seen on January 1st, in excellent health. Left arm perfectly normal. Directed to quit carbolic acid.

Galveston, May 1, 1893.

Both patients fell back again into their former condition, after the treatment had been stopped. The improvement lasted for about five months, after which time they gradually became worse. Patient II. improved again afterward, and is in very good condition. Patient I. has been operated on with excision of degenerated cortex. Result not yet known.

movements of the right hand. The attacks gradually grew severer until full-fledged convulsions set in, begining with twitchings and contortions of the right hand, which symptoms later manifested themselves in the arm and leg. Had spells every two or three days. Memory remained good, but his mental capacity became dulled and depressed. When he had attacks he wanted to run continually. With staring eyes he would overcome all resistance and force himself to a door or window. He, therefore, had to be watched continually.

An examination showed old cicatricial marks on either side of the head, one over the left motor zone, the other over the left parietal bone; both producing pain upon percussion.

I operated on him in July, 1891. I lifted up a large bone-flap (Wagner's operation) and met a very soft, round spot of about 2″ in diameter, of a more yellow-reddish color than the adjoining gray mass, which to the finger conveyed the impression of a hard ring all round. A fine knife was inserted to the depth of about 2″ into this soft mass. According to an understanding with the boy's father, and also because a removal would have had to reach deeper down than seemed advisable, nothing further was done. The bone flap was replaced with the exception of a piece in its middle line, which accidentally broke out, and which opening seemed desirable anyway as a safety valve. The patient made a rapid recovery so far as the operation was concerned, and went to his home in the country, from where, for the first five or six weeks, I received the most glowing reports. After that he again got worse and continued to get worse than ever before.

In May, 1892, he returned for treatment. The father reports light convulsions of the right side and loss of consciousness almost every day, sometimes even two or three daily. The spells are harder every third or fourth day, when the convulsions are very severe. His mental condition is very deplorable. He has a stupid, staring look, is unable to do anything, and from time to time has a wild desire to run away. Has also aphasia during the attack. He is almost helpless mentally, complains of headache, etc. Ophthalmoscopic examination by Dr. Hall shows some hyperæmia and blurring of the outlines of the disks—no choking or distinct swelling. Knee-jerk is exaggerated on the right side.

On May 12th I lifted the scalp from over the opening left in last year's trepanation, so as to be able to apply the current to the dura mater, which was pulsating nicely. After this the spells were less severe until the 17th, when he had one as bad as ever.

On May 18th, I first applied the negative pole to the brain and the sponge to the foot and hand, using 5 to 9 milliampères for ten minutes. Slight convulsive sensations.

23*d*. First severe spell.

25*th*. Applied the positive pole to the brain.

28*th*. Another hard spell. Current every other day.

June 5. Very hard spell. The patient feels better immediately after each application. His mental condition has changed so wonderfully that he feels like a healthy being. Heretofore he had to be led to the office for treatment, whilst now he is self-depending, walks by himself, and is able to attend to his business.

7*th*. He had a bad spell, loss of consciousness, convulsions.

8*th*. Light spell, no loss of consciousness, no convulsions.

9*th*. Very light spell, no loss of consciousness, no convulsions.

13*th*, A.M. Very light spell, no loss of consciousness, no convulsions, and P.M. had a very hard spell, loss of consciousness, and convulsions.

He has frequent petit mals and a grand mal about every fourth day. But he had only one hard and one mild spell from June 18th to July 3d. Has frequent premonitions. The patient now complains of severe pain over the posterior scar left by the operation, and corresponding to the posterior part of the middle portion of the parietal lobe. The place had always been tender, but never so much so as now. The spot is very painful on palpation.

July 4. The scalp over the tender spot is lifted up after a crucial incision, since the scar was suspected as the cause of the trouble. No improvement.

8*th*. A button is removed with the trephine. The dura bulges out, is deep blue, and has no pulsation. The trephine hole is enlarged with plyers. The dura is attached to the skull all around the opening, but is easily detached by the finger. The cystic protrusion of the dura is incised, and dark blood mixed with pieces of brain pours out. The dura is slit and an area of brain 2″ in diameter is found disintegrated and broken down, as if having been subjected to heavy pounding. A good deal of

it sloughs away. Hemorrhage severe. Iodoform gauze plug inserted. Patient rallies well, but the right arm and leg are paretic; the fingers are perfectly paralyzed. Aphasia. Understands what is said, but looks as though everything were new to him—plays with my watch-chain, raises his arm for the thermometer, etc.

10*th.* The gauze is removed; the soft brain prolapses. Gets feverish and dies on the twelfth day from encephalitis.

Post-mortem examination, which had to be made in a hurry, at night, immediately after death, and which was conducted by Dr. Hendricks, showed a cavity filled with débris of brain substance and dark blood over the upper and middle part of Rolando's fissure about 3″ in diameter, running forward and downward into the third frontal convolution. The walls of the cavity were dark and infiltrated with blood, like in an old abscess. Most likely it was a breaking down of brain substance following a severe concussion and perhaps aggravated by the manipulations of the first operation, sounding, etc. Such a case would prove that the brain may be severely injured without fracture of the skull—by concussion.

In concluding my report I deem it my duty to express my gratitude to Dr. J. E. Burk for his valuable advice in the use of the electrical appliances, and to Drs. C. H. Wilkinson and A. C. Smith (U. S. M.-H. S.), for their ever-ready assistance in the operative part of my experiments.

ABDOMINAL HYSTERECTOMY WITHOUT A PEDICLE.

By Rufus B. Hall, M.D.,
Cincinnati.

The treatment of the pedicle in supra-vaginal hysterectomy has been the bone of contention with operators for many years. There have been two great divisions, one advocating the intra-peritoneal, and the other the extra-peritoneal treatment of the pedicle. For a number of years, one or the other of these methods has been recognized as the orthodox plan, and of these, the extra-peritoneal is the one most frequently employed. For this reason, it will be the method most frequently used in making comparisons in this paper. As we might naturally expect, there have been many modifications and improvements in the minor technique of Schröder's and Péan's methods as originally practised by them. As our knowledge of the pathological conditions and complications to be overcome in removing fibroid tumors increased and our operative experience became broader and more mature, the Péan method of extra-peritoneal fixation of the pedicle became the one which was almost universally employed. The reason of this is obvious when we recall the fact that only a small percentage of all fibroid tumors are suitable cases for treatment by the Schröder method by those advocating it, and of these it was not possible to control hemorrhage in all cases, and, above all, the Péan method gave the best results. Those of us who have made many abdominal hysterectomies for the removal of large tumors know that it is not an uncommon thing after opening the abdomen to find one ovary high up and easily managed,

and the other adherent behind the tumor in the pelvis, or the tumor descends into one of the broad ligaments and body of the uterus so far that you must separate it from its peritoneal envelope before a suitable pedicle can be made and fixed outside; manifestly these cases are not suitable ones for the intra-peritoneal method even by the most ardent advocates of it, and must therefore "heretofore" be treated by the extra-peritoneal method. But, thanks to the ingenuity and skill of American gynecology, we have now a method which promises all of the advantages of both the old methods with but few of their disadvantages. It was one of these desperate cases in which the operator had to "enucleate large nodular masses from the broad ligament" that induced him to remove the entire cervix in his first total extirpation operation. The success in this case encouraged him and others to make the operation and improve its technique and to give their methods and results to the profession upon its merits, and it is upon this basis we ask a hearing before the profession at this time. The advantages over all other methods are:

First. There is no pedicle to become necrotic, and thereby a source of septic infection or fatal hemorrhage if the intra-peritoneal method is employed.

Second. There is no more danger from hemorrhage than after ovariotomy.

Third. There is no raw surface left in the peritoneal cavity to favor intestinal and omental attachments.

Fourth. It is no more difficult than many other abdominal and pelvic operations, and does not require any more time to perform it.

These are a few of the advantages of the method as suggested to me in my experience with it. We are all aware of the fact that if we use the intra-peritoneal method we are in great danger of losing our patient from intra-abdominal hemorrhage, and if we use the extra-peritoneal method we not infrequently see the pedicle slough and become a menace to the life of the patient for days afterward, and not a few die from septic infection from that cause. If the patient makes a

primary recovery from the extra-peritoneal method she is not in all cases restored to health. She not infrequently suffers great pain due to contraction of the pedicle dragging upon the abdominal scar, and pressure upon the distorted pelvic organs, interfering with their functions because of the fixation of the pedicle, and not infrequently hernia follows this operation. Again, the prolonged and painful convalescence which necessarily follows this method is no slight objection to it. If we can remove these objections without additional risks to our patients it is our duty to do so. The total extirpation method overcomes these difficulties. There is no danger of hemorrhage. There is no sloughing pedicle. There is no distortion of the pelvic organs from the stump being fixed to the abdominal wall pressing upon the bladder, or interfering with the function of the bowels, and we have a comparatively painless convalescence, which is shortened at least two weeks in duration over that of the extra-peritoneal method.

By the total-extirpation method there should not be as much danger of hemorrhage as there is in simple ovariotomy, from the fact that in the latter operation the pedicle is transfixed and ligated in mass, and in not a few cases the pedicle is short and thick with great tension upon it, favoring the slipping of the ligature and hemorrhage, while in total extirpation the broad ligament is divided from the uterus, ligated in sections which are not put upon the stretch ; therefore there is but little, if any, danger of a ligature slipping off. The ligatures do not include any uterine tissue, therefore the tissue within their grasp is not susceptible to undue shrinkage and resulting hemorrhage.

There is no raw surface left in the peritoneal cavity after the operation is completed to form attachments to intestine or omentum. After ligating off the ovaries upon both sides, the peritoneum only is divided about one and a half inches above the top of the bladder across the front of the tumor, and at a corresponding height on the back of it. This is done before any temporary clamp is placed. The peritoneum is then stripped down in front, and the bladder separated from the tumor down to the vagina, and then the peritoneum is stripped

from the tumor behind and from both of the broad ligaments, and all of the ligatures which are required in the subsequent steps of the operation are placed below or behind the peritoneum. The number of ligatures in any of my operations has not exceeded three upon each side after the peritoneum was stripped down. The ends of these ligatures are left about six inches long, and after the cervix has been removed are carried out through the vagina, and are cast off or removed through that passage. The peritoneal edges which were stripped from the tumor are now turned in toward the vagina and neatly coapted by a running stitch of catgut, thus closing off entirely the raw surface in the vagina from the peritoneal cavity. The wound in the vagina is treated as after an ordinary vaginal hysterectomy.

The difficulties attending the operation are not so great as one would suppose who has not attempted it, and are easily and rapidly overcome by one skilled in difficult and complicated abdominal and pelvic operations, and the time required to perform the operation is no greater than after many of the difficult abdominal operations now being performed daily.

By removal of the cervix it has been said that the key of the arch of the pelvis is destroyed and a weak point is the result, which will, in a certain percentage of cases, result in vaginal prolapses or hernia. Such an accident has not followed the operation to my knowledge, neither has it occurred after vaginal hysterectomy; and I do not believe it would occur any more frequently after abdominal hysterectomy than after vaginal hysterectomy, where the two conditions after operation are identically the same, and we do not hear objection to the latter operation on that account. The method has stood the test of experience admirably, and theoretically there is little to be desired in technique, as it is very near perfection. In patients with thick abdominal walls this method promises as good results as in those with thin abdominal walls, which cannot be said of the extra-peritoneal method. I am convinced that the method has come to stay, and that the clamp in abdominal hysterectomy will as certainly be a thing of the past as it is now a thing of the past in ovariotomy.

LACERATION OF THE CERVIX UTERI AS A FACTOR IN PRODUCING PLACENTA PRÆVIA.

By T. Grange Simons, M.D.,
Charleston, S. C.

The subject proposed is one that has engaged my attention for some years, and I will endeavor to substantiate my theory by referring to three cases occurring in my own practice, briefly alluding to them, and only so far as relates to our subject.

Case I.—Mrs. H., multipara, with a history of five premature deliveries from the fifth to eighth months of utero-gestation; resulting, as was stated by former physician, from laceration of the cervix uteri. I saw the patient first July 30, 1884. She was then about the seventh month of pregnancy. She had some slight pains with some hemorrhage. Rest and fld. extr. viburnum prun. were prescribed. August 3d a sudden flow of blood, without pain, occurred. I found, upon examination, a large, old laceration that extended up the vaginal junction, the os undilated, the cervix obliterated; the hemorrhage was not great until August 9th, about 8.30 a.m., when it was profuse. I found the os dilating and a soft, fleshy mass presenting; patient in good condition. I tamponed firmly. At 11 a.m.; bleeding recurred, removed tampon; dilatation had advanced. I now determined to detach a portion of the placenta, and liberated the right segment of a centrally placed placenta; free bleeding ensued. I now drew the detached portions through the cervix and doubled it over upon itself, with the cervix between the folds. I kept it firmly compressed with my three fingers within the uterine cavity, rupturing the membranes. The head engaged, and although my patient

was very weak from loss of blood, she was still brave and self-reliant. Hemorrhage was controlled by the compression. The child, exsanguined and dead, was delivered at 12.30 P.M. Ergot was used hypodermatically on account of nausea.

CASE II —Mrs. T.; seventh pregnancy and between the sixth and seventh month. Has a history of placenta prævia previously that nearly terminated her life. Was seen on the night of August 7, 1885. The patient was awakened by a free hemorrhage, and found presenting from the vulva a pouch of membranes. She attempted to replace them, but desisted, as the attempt increased the pains. I found her in good condition. I ruptured the membranous pouch, and the umbilical cord was found contained. Examination showed that a disk of placenta was firmly attached over the larger circumference of the os, and that the presentation was transverse. The bleeding was quite free. I quickly detached enough of the placenta to double it over and out of the os, and brought down the feet and delivered a small child at 1.40 A.M. Ergot was given, but she vomited it was then injected hypodermatically. The placenta remained firmly attached. The patient became very faint and nauseated when I attempted to remove it. Uterine action was very weak, Credé's compression used, but also caused great pain, nausea, and prostration if much pressure was exercised. The bleeding was quite free, but some uterine contraction occurred, and the placenta was still further detached. I found very firm adhesions under the insertion of the cord. The placenta was delivered and uterine contraction secured. There was an old and extensive laceration of long duration.

CASE III.—Mrs. G., multipara, with three children, and history of three premature deliveries attended by profuse bleedings, repeated at periods of weeks and lasting for days. Now nearly four months pregnant. Has been bleeding for several days. Extensive laceration existed for several years, but operative measures had been declined. When examined I found what I thought to be an extrusion of the fœtal membranes through the os, and attempted to turn the mass out with my fingers, but found it firmly attached within the uterus and the adhesions extending into the cleft of the lacerated cervix—virtually an ectopic gestation. Sims' speculum showed the ovum firmly attached, with large bloodvessels extending from the attachments

and over the laceration. The mass was expelled and proved to be a twin conception.

The opinion I hold is, that such lacerations tend to produce the condition of placenta prævia. The whole uterus is kept in a state of hyperæmia by the injury; and, further, that endometritis results in a degree not always sufficient to prevent conception. Also, that the change in the shape of the uterine cavity produced by the laceration and the inflammatory processes resulting from such laceration; and that such changed contour of the cavity tends to favor gravitation of the ovum to the cervical zone and its attachment over or within the cervix uteri, and in the case mentioned, No. III., in the spread of the fœtal membranes, and that the villosities found ready attachment to the vascular surface of the old laceration. The frequent recurrence of premature expulsion of the contents of the gravid womb by women with neglected cervical tears has been observed by all of us. May not this tendency to abortion be aggravated by the changes in the uterine cavity and of the increased vascularity of the whole organ, but especially in the cervical zone; this is due to the prolonged irritation produced by the laceration. Such injuries do not prevent conception, for we note the frequency of the recurrence of pregnancy and its frequent interruption by abortion. This was illustrated in the three cases I allude to. We are taught that in the early stages of pregnancy in a healthy womb that the cervix is but slightly hypertrophied, and that it is less abundantly supplied with blood than the body of the uterus, and that the cervix is not subjected to the shock or stimulus, owing to the presence of the growing ovum. The intra-vaginal portion of the cervix also loses its firm consistence and becomes softer as pregnancy advances; these changes are arrested or prevented by the presence of an old unhealed laceration that keeps up a chronic state of irritation, with its consequent hypertrophy and hyperplasia. True, the process of softening is less in the multigravida, and that frequent pregnancies do tend to produce chronic inflammatory conditions in the cervix and endo-

metrium. Also, that the cavity of the uterus is changed in contour by repeated pregnancies, and that we do have a greater tendency to placenta prævia in those who have had frequent and rapidly repeated pregnancies, and that the condition is rare in the primigravida. May this not be due to the fact that in the primiparous woman the healthy condition of the uterus, and especially the normal condition of the cervix, prevents the tendency to a descent of the ovum and its subsequent attachment within or near the cervix, where in the normal cervix it will not find sufficient blood-supply to sustain it. The frequent existence of cervical lacerations may be potent causes in producing placenta prævia, for we all know how often such lacerations exist, and how comparatively seldom do women submit to treatment for their abatement, although frequent abortions and premature labors occur, with all the dangers and restraints imposed by such conditions; yet, they are borne in silence and operative measures declined.

In my search for the literature of the subject I have found but little that will substantiate my theory. In a paper read before the American Gynecological Society, by Dr. Ed. Warren Sawyer, of Chicago, "On Partial Rotation of the Ovum as a Cause of Placenta Prævia" (see vol. xiv. 1889, p. 317), he speaks of a partial detachment of the ovum from its normal site, and alludes to two cases—one a primipara who jumped from a carriage drawn by runaway horses, she then being pregnant about five weeks, this followed by uterine hemorrhage; pregnancy was continued to term, but placenta prævia was the condition. The other case, a multiparous woman, who, in the early weeks of pregnancy, stepped off a chair and received some shock; this case, also, was one of placenta prævia. He claims that the ovum may be partially detached and rotates upon its axis and forms new adhesions within the cervical zone. The point of such contact becomes the placental site, and thus placenta prævia is produced. The permanent condition of traumatism that exists where there is an unhealed lacerated cervix, with the changes produced in the shape of the cavity, all tend to produce a condition favorable

to abortion; but should the ovum be arrested and become implanted within the cervical zone, we have a cause for placenta prævia. True, the irritation is aggravated by the hyperæmia already existing, and pregnancy is most often interrupted in the early weeks, and that such premature interruptions of pregnancy are usually attended by profuse hemorrhage.

I submit this paper for a free discussion, only with the view of eliciting the opinions of those so well qualified to discuss the matter, and perhaps future observation from them may tend to confirm or reject my views. As it is rare for one physician to have many of such cases, the combined experience of those present will be valuable.

TRAUMATIC INJURIES OF THE HEAD.

By John T. Chapman, M.D.,
Bessemer, Ala.

It is with no little diffidence and apprehension that I offer this Society a paper which is a theme that is being handled by men of more experience than I can boast of. By each of us bringing the fruits of his labor and offering it at the altar of the profession we will reach the goal for which we are striving. I do not come before you with the promise of presenting new, and, therefore, interesting facts, yet all of us are earnestly looking and longing for them. Every statement, proposition, or theory in any department of medicine, before it can be reckoned among settled facts, must be submitted to a calm and judicious consideration publicly and openly before the medical profession. It is hoped that it will elicit a full and frank interchange of experiences, and thereby help in some measure the progress of medicine in this department of surgery.

All traumatism involving brain injuries were for many years classified as cases of concussion or compression. The classification was undoubtedly simple and easy of comprehension. If the intra-cranial space was diminished by the intrusion of bone, serum, extravasated blood, or pus, it was compression; otherwise all other brain symptoms were referred to concussion. I think that compression and concussion should be regarded as one, the one of more intense and of greater brain-lesion than the other; but if these terms be discarded (for they have a tendency to mislead one), and were used instead such terms as laceration, general contusion, or fracture, with

hemorrhage, we could more accurately determine the character of lesion than if our attention be directed solely to a symptomatic condition that may not clearly exist. Fractures of the skull without complications are not only without importance but they are devoid of symptoms. A simple fracture over the vault is often overlooked in the absence of pressure symptoms, and even when there is a fracture with spiculæ, the extravasation of blood often produces the most alarming symptoms. I give two cases in detail.

Case I.—Mr. M., a man of exceptionally bright mind, a picture of physical health, and weighed about 190. He was out riding, when the team became frightened and began to run; to save himself, as he thought, he jumped from the vehicle, which was a side-bar surrey, and in consequence thereof landed upon his head; he was somewhat dazed at first, but, soon recovering himself, he got up and walked home, about six blocks away; after resting awhile he began to complain of soreness of the muscles, and said that once in a while sharp pains would pass through his brain; his friends thought it advisable for him to have a physician called, but he insisted that his injuries were not of sufficient seriousness to have one; he retired early, and, as his friends thought, dropped off in a quiet sleep; no attention was paid to him until next morning, when someone passing through the room noticed that his breathing was laborious, so they tried to arouse him, but finding that they could not, the family were notified and I was sent for. On my arrival I found him in a semi-conscions or comatose state, no recognition when aroused by shaking; he would raise up, look around, and lay down; there seemed to be no appreciation of objects, or pain, or feeling. Upon close examination I found two contused wounds, one a little above and superior to the left ear, no abrasion; the other on a line two and a half inches superior to the occipital protuberance, this the larger of the two; while there was no decided cut, the wound bled freely; there was no paralysis, whole or partial; eyes normal; pulse rather slow, 62; respiration laborious; temperature 99½°; locomotion was not impaired, as in the later part of the day; his bladder became distended, and wanted to relieve itself; he roused up from this semi-conscious state,

got up and went to the bidet (through force of habit), which was in its accustomed place, and passed his urine, also relieving his bowels. The reason that I claim it was mechanical with him is, I removed the bidet to another part of the room, and the next time his bladder wanted to relieve itself he went to the accustomed place, and would have passed his urine upon the floor had I not placed a vessel to catch it.

My diagnosis at this time was compression, caused from hemorrhage, as there were no signs by which I could locate a fracture. With these facts before me, and there being no immediate danger to life, I decided that operative procedure was unnecessary; I used cold applications to the head, such as cold cloths made cold by laying on ice. Finding that they did not retain the cold for any length of time, I used the ice-bag. This was to control the inflammation which I feared had already begun, as the temperature had increased to 102°; large doses of iodide of potash every few hours, to absorb the hemorrhage; liquid diet was given, such as milk, beef-tea, etc.; this treatment was continued until the third day, when his symptoms began to improve; on the fourth there was some recognition, and he would try and express his thoughts, or ask for what he wanted. On the fifth there was aphasia, Brocas speech centres and the isle of Reil were involved, as indicated by the aphasia; he understood everything that was said to him at this time, but could not regulate his speech movements, and was painfully angry if asked questions or requested to repeat; later on there was paraphasia, for there were certain words that he could not pronounce and sentences that would be mixed and confused; when asked to write the alphabet, he would get as far as C, stop, rewrite them until twenty-six had been written, and never going beyond C. I noticed at this time that the sensitive filaments of the pneumogastric nerve were affected. This was caused from pressure on the lower floor of the fourth ventricle. The sensitive filaments are to be traced deeply through the fasciculi of the medulla, to terminate in the gray nucleus near the part of the lower floor of the fourth ventricle; this nerve is composed of both sensitive and motor filaments; it supplies the organ of voice and respiration with both motor and sensitive fibres; the pharynx, œsophagus, stomach, heart with motor influence. With these facts before me, I came to the conclusion that the traumatism was from the

injury back of and superior to the ear; still, thinking it compression from blood clots, I deferred operating, as the patient seemed to be improving. On the night of the 6th, after traumatism, to my surprise and regret, he was seized with convulsions; they would come on with great force, and last at least thirty minutes. I advised an operation, stating that it should be done immediately. I was of the opinion that an operation, if it should fail, could not do as much harm as the convulsions; that by removing a button or two I would restore equality as to pressure, relieve the resistance at given point, which would relieve convulsions for some days; if the pressure was located, which I was pretty sure it would be, the convulsions would be relieved immediately, and consciousness restored. Having stated these facts to the family, an operation was agreed upon. Everything was gotten ready for such, and two assistants were sent for. On their arrival, one refused to give the anæsthetic, stating that his reasons were, the compression was so intricate, the locating so doubtful, that an operation would not be justifiable; also the seeming quietness of the patient at the time led him to the false impression that the convulsions had ceased, so the operation was postponed.

Early the next morning I received a call to hasten to Mr. M., stating that the convulsions had returned and that they were willing for an operation. Everything having been made ready the night before for the operation, he was anæsthetized (ether), placed on the table, and two buttons were taken out where the injury was received.

After dissecting up the scalp and cleansing the skull I noticed a very fine fracture; the buttons were removed, and upon examination I found a large spicular bone penetrating the dura mater; the dura was opened and four ounces of coagulated blood removed; the opening was nicely washed with hot water, drainage-tube inserted, the edges of the wound brought in close proximity by means of silkworm-gut, sprinkled with iodoform, and dressed with iodoform gauze. He came out from under the ether very nicely, but was very restless; I administered one drachm of bromide potass. by enema, which acted very nicely; this had to be kept up for several days, as at times he would be almost unruly. From the second day after operation his improvement was gradual, and on the twenty-seventh day after operation he was dismissed.

Having decided to operate, the procedure and technique at once demand careful consideration. It is in this line of surgery that asepsis must be rigorously insisted upon; be sure of the aseptic condition of the surroundings, of the instruments, ligatures, and dressings, of everything that comes into immediate contact with the patient; and, most important of all, be sure of yourself and your assistants. The head is to be shaven—no half-way measure. It is then carefully cleansed, the sebum and everything removable is to be thoroughly scoured out with soap and water, followed by disinfectants.

The main fissure is to be marked out with an aniline pencil or silver solution, as recommended by Church. With a trephine or drill the scalp is to be transfixed at the exact point decided upon for the entrance of the skull opening, and the bones scored to secure a marking for the operator when the scalp shall be uncovered and the guiding line on the scalp removed. With a wide horseshoe flap embracing the periosteum, the scalp is turned back, the base being upward in relation to the patient's recumbent position, to favor subsequent drainage. Bleeding-points may be controlled by hæmostatic forceps, the points, previously marked out by drill through the scalp, are now found, and the bony structure carefully examined for evidences of traumatism. You are now ready to do the trephine, removing as many buttons as necessary; if more than two buttons be removed, advise the use of the drainage-tube, wash the wound with hot water, examine the dura mater for injuries or spicular bone, place in drainage-tube, bring the edges of the scalp in a position by means of the interrupted suture, sprinkle with iodoform or boric acid, dress with iodoform gauze and borated cotton. By strictly adhering to this technique the operation is made safe from septic germs.

In these days of modern surgery, where wounds can be made surgically clean and caused to heal almost by first intention, it is my opinion that in every case of traumatic injury of the head, where convulsions come on, the patient should have the benefit of the trephine, to remove clot, or spicular

bone that has been located where there is pressure, for the same object as the abdominal surgeon makes his incision in the abdomen for the purpose of exploration.

I believe that the day is not far hence when this will be done, and when that is so we will see fewer cases of epilepsy from traumatism.

Case II.—Boy, fourteen years old, who, some three years previous, received injuries of the head, produced by something falling on and fracturing the parietals. For three weeks after injury he had been suffering from epileptic convulsions; they had become more frequent and with more force; operation was advised and done; the technique the same as reported above. Two buttons were removed at the point of injury. There was considerable pressure from spicula, the dura indurated and hard. The induration was cut out and edges of dura brought together by the continuous catgut suture; drainage and dressing as recommended. The wound healed nicely. There has been no return of convulsion, and sufficient time has elapsed to show conclusively that he is entirely relieved.

The two cases illustrate the brilliant possibilities of the surgeon to relieve this class of cases that, if not always fatal, will destroy the beautiful temple of thought and dethrone the ruler of physical man.

A CASE OF OBSCURE ABDOMINAL HEMORRHAGE.

By I. S. Stone, M.D.,
Washington, D. C.

N. N., colored, aged twenty-five years, III.-para, came to Columbia Hospital in August, 1892, having been under observation in the dispensary for about two months. She had first menstruated at fourteen, had been regular, and had scanty and painful flow. She had three times miscarried, the last several years ago. Soon after entering the hospital she was examined by the staff, who agreed that an operation should be done to ascertain and relieve obscure pelvic and constitutional symptoms. Her abdomen was somewhat distended, although its size did not attract much attention. She was anæmic, but declared she suffered no great pain or distress. An obscure mass was discovered filling the pelvis, but not involving the uterus. Two days prior to operation I discovered an increase of the abdominal distention, which was not relieved by the usual purgation given all patients before abdominal section. As no one of the abdominal organs was found enlarged or misplaced it was impossible to ascertain the cause of the distention or the very decided anæmia of the patient, who by this time was anxious for operation, as she believed herself ill. Her pulse was rather weak and quick. Temperature 100° the day before operation.

Operation, September 3d, 1.30 P.M.—When the abdomen was opened blood in large quantity poured out. In short, the abdomen was full of blood, and the operator fully expected the patient to collapse at once. The appendages on both sides were quickly caught up and clamped with forceps, with the idea that hemorrhage might be from one or the other of them. But such was not the case. A small opening was found posterior to the uteurs

into what seemed a sac, and quite naturally I concluded that I had a case of abdominal pregnancy. On careful inspection of this sac, after removing the blood, there was no sign of placenta, fœtus, or even placental site. The usual glistening peritoneal lining of a normal cul-de-sac presented itself; the opening I had found proved to be a separation of an old adhesion between the rectum posteriorly and the lower segment of the uterus and adjoining broad ligaments. There was absolutely no other pathological condition found within the pelvis or abdomen, although careful search was made. Ten gallons of water were required to remove all the blood from the abdomen. Only a few small clots were found. The blood was venous in appearance, and was, unfortunately, allowed to escape into a vessel containing water, hence could not be measured. A quantity escaped upon the table and floor of the room, which turned quite a bright red from exposure to the oxygen of the air. It is safe to say that enough blood escaped to fill an ordinary water bucket. A gauze drain was left within the lower end of the incision, extending to the bottom of the cul-de-sac. The patient was put to bed in fairly good condition, and gradually and safely rallied from the shock of the operation, making a complete recovery.

Dr. D. S. Lamb, pathologist at the Army Medical Museum, examined the ovaries and tubes removed from this patient, and says in his report, "There is nothing about the ovaries or tubes to indicate a recent pregnancy." One of the ovaries was enlarged, owing to cystic degeneration, but there was no other pathological condition of the appendages.

POISONING BY THE BITE OF THE SOUTHERN SPIDER.

By J. T. Wilson, M.D.,
Sherman, Texas.

The so-called little things in surgery are not now receiving the attention they deserve, and are overshadowed by the capital operations in the respective fields of surgery and gynecology, and the surgical and gynecological minds in recent years have been so absorbed in these graver troubles that the minor accidents so frequently met with are given but little attention, either in principle or detail, and sometimes altogether ignored. Not that I for a moment underrate the capital operations and the graver conditions calling for them, nor find fault with those who give their whole time and attention to them, which I think all important and necessary, and to which time the best thought of progressive men should be directed; I also appreciate the inestimable amount of good that has been accomplished in recent years along this line, but I do not think the so-called minor accidents should be neglected and run over as not worthy of attention, because often from them the more serious troubles spring. For this reason I thought it not inappropriate to call attention briefly to the subject of this paper, viz., poisoning from the bite of the Southern spider, which is more frequent and often more serious than is generally believed by the profession, except those members who have been called upon to treat them.

The literature upon the subject of poison wounds inflicted by the spider is scant in the extreme, and some even deny that any virulence is attached to the bite of the spider.

Neither are the poisonous wounds inflicted by the tarantula—which is but a larger species of the spider—nor the centipede, both of which are so common in the Southern States, and the stings of which make such painful wounds, sometimes producing death, more than casually alluded to in a few brief sentences in the text-books.

There are few practitioners in Texas who have not been called upon to treat wounds caused by the poison of a spider bite. The species to which this paper is especially designed to consider are very small; they belong to the order of arachnids, which also comprise the tarantula, centipede, and scorpion, all of which flourish in great profusion in all parts of Texas, but especially in that region round about San Antonio, and all the way to the Gulf on the south and the Rio Grande on the west, a region of country subject to great and prolonged droughts.

It is well known there that these insects inflict painful and often very serious wounds, which they poison somewhat after the manner of the rattlesnake, by fluid secreted from glands situated upon each side of the caput and retained in a sac at the roots of the falces, and in the act of striking the muscles contract over the sac and project it through the channels in the falces and fangs intended for the purpose, and thus into the wound.

The following brief description of this process and the anatomy, from the *Encyclopædia Britannica*, is very interesting: "The poison is secreted into two small elongated sacs, one for each of the falces situated within the cephalo-thorax on either side of the alimentary canal. The sacs are connected with a membranous channel in each of the falces, the channel running completely through the fang from an orifice, near the end of which the poison is pressed out by the spider in the act of wounding its prey. The falces are two in number, articulated immediately below the caput, opposite to each other and armed with teeth, each also terminates with a movable curved fang which, when not in use, is folded up. These fangs are internally channelled and perforated at the extremi-

ties through which the poison is instilled into the wound made by them."

It is surprising what extensive and painful wounds these little insects can make. In my native State, Maryland, I never heard of any such trouble from this source, nor in Missouri, where I practised some years. I therefore presume they are only known in the more Southern States. They are of the araneidea family of the arachnid species, though their exact position in the group has not been clearly and well defined, and the natural history, the correct division and classification of the arachnids of Texas have never been thoroughly studied so as to place them in their natural order. Their bodies are very small, not so large as the common pea, and in shape, number of legs, caput, falces, fangs, etc., resemble somewhat the larger species, the tarantula, though their color is not so black, nor are they so hairy as these latter. There are two species of these little spiders; the immediate effects and results of the bites of each are different. One is of a dark brown or muddy black color, the other in size and shape almost a duplicate of the above, except in color, which is more of a dark-grayish; these have a distinctly visible but very small white spot in the centre of the back, while the darker species have a red spot in the same situation; the latter are more numerous, and from what I can learn inflict the greatest number of wounds. These spiders mostly inhabit dwellings, barns, stables, water-closets, and other out-houses, wood, lumber piles, old fences and like places.

Deaths have been reported as a result of wounds made by these insects, though I have no personal knowedge of such a termination.

The wound inflicted by the little gray spider is by far the most painful in the acute stage; the pain, beginning at the time of bite, increases until it becomes intensely acute within a few minutes. The patient becomes cold, drops of perspiration roll from the face, respiration is impeded, the pulse becomes rapid, small and feeble, nausea and vomiting sometimes occur, a condition of collapse supervenes, and it would seem

that if relief did not come quickly death would ensue. A very minute dark spot, surrounded by a white areola, slight though perceptible swelling, is all that can be seen upon the surface where the wound was made. This gradually passes away, and in a few days nothing can be seen, although a feeling of soreness and stiffness of the immediate parts will remain for a day or two after. For two or three days, sometimes longer, a period of prostration and languor continues; in most cases, however, all the symptoms soon pass away, and no effects of the wound are felt or seen.

The wound inflicted by the darker insect is not so painful; sometimes it is described simply as an itching sensation, followed by soreness and stiffness of the part bitten, but a larger spot is seen, made by the fangs, and is usually red. The symptoms are not so rapid as in the other variety, coming on more gradually and with less pain, but a greater amount of swelling ensues. There is never a great deal of acute pain complained of, but there is always a considerable degree of soreness and stiffness of the muscles, which is constant, and amounts to a very annoying aching. In a few days, immediately around the central spot there is beginning discoloration, at first dark reddish, then changing to a bluish color. This progresses very slowly, and is sometimes many days developing; finally a black gangrenous spot of the dry variety supervenes, which continues unchanged, sometimes for weeks; then slight suppuration takes place, but its progress is very slow, and the skin, cellular tissue, and muscles seem to exfoliate in flakes and with but little discharge. I have seen these wounds two inches in depth and larger than the palm of the hand, upon the sides of the calves of the legs and in other places, though singularly enough most of the wounds from these insects that I have seen were on some part of the calves of the legs. I have known patients to suffer for six and ten months, necessitating at first a crutch and cane in order to get about, and then a cane for assistance, having considerable limp all this time, and suffering more or less on locomotion. Of course, any movement that would bring the

affected muscles into action would cause the pain and at the same time retard the healing of the wound.

A professional friend related to me the case of a beautiful little girl, eleven years of age, coming under his care who had the misfortune to lose one of her ears from the bite of a spider; the gangrene invaded both muscle and cartilage, the entire organ sloughing away down to the periosteum of the temporal bone.

In spite of all remedies, these gangrenous spots would seem to remain unchanged for months at a time, or succeeded by unhealthy granulations that were not disposed to heal, the general health apparently not specially affected by it, except during the more acute stage, when there would be loss of appetite, constipation, and some malaise.

In regard to treatment. The wound inflicted by the dark gray spider, in which the pain is so great, demands full doses of anodynes, morphine by hypodermatic injections without stint, chloroform by inhalation, diffusible stimulants, especially brandy and ammonia, local applications to the part of ammonia and laudanum, camphorated soap liniment, solutions of boric acid and laudanum, or solutions of chloral in alcohol and spirits of rosemary make good applications. When pain is relieved a good active cathartic combining a mercurial is attended with good results. Especial attention to diet is of great importance; the stomach should not be overloaded, and indigestible substances should be carefully avoided. The digestion seems specially affected in many cases. This course has been very prompt in meeting the demands of the cases that have come under my observation.

Wounds from the other variety of spider do not call for such treatment, as the symptoms are not so urgent; anodynes are not always demanded, but prompt local treatment, if the case is seen in time, will save much future suffering and tissue also. If the wound was made upon a part that can be constricted by a ligature, this should be done at once, and the parts thoroughly bathed in a hot boric acid solution, then a hypodermatic injection of a few drops of cocaine given, followed by scarification, then cupped, and finally a solution of

permanganate of potassium injected into the parts, after which the ligature can be removed, and local applications of ammonia and antiseptic dressings, especially bichloride of mercury, be applied.

If the treatment can be applied in time the resulting troubles can in many cases be obviated. If, however, the case is not seen until the tissues have been saturated with the poison, which has an acid reaction, and inflammations and swelling have supervened, a soothing course of treatment, with alkaline applications, should be instituted. Hypodermics of ammonia or permanganate of potassium sometimes do good here also. Hot poultices, if much redness, stiffness and swelling are present, often give much relief. The dressings should all be thoroughly antiseptic, the underlying muscles and surrounding tissues if possible kept at rest. It is necessary to look after the general health and maintain the secretions in good condition. Tonics, attention to diet and general hygiene are important and useful aids. If sloughing begins with a gangrenous tendency, as it is so liable to do, the most satisfactory and I think the best procedure is to anæsthetize the patient and thoroughly excise all the diseased structure, make as near as possible an aseptic wound, and then treat it antiseptically. This, if well done before the tissues form too large a space surrounding the wound are involved, will save the patient much suffering, time, and trouble. One great advantage in this course is that you can control the patient until the wound heals, which is of much importance; otherwise it is difficult to keep him quiet, and all movements retard the granulating process.

The treatment as outlined above has served me best in cases that I have seen, as also in the hands of some of my professional friends; but it is not always satisfactory, and sometimes cases progress very slowly with it. There is room for investigation and thought in this field. If some remedy could be discovered that would neutralize the poison of these venomous little animals, prevent destruction of the tissues, and bring the case to a more speedy termination, a valuable addition to surgery will have been made.

INTESTINAL ANASTOMOSIS WITHOUT MECHANICAL DEVICES.

By John D. S. Davis, M.D.,
Birmingham, Ala.

Anastomosis is unquestionably the best means for the restoration of gut calibre. No method of bringing gut-ends together has been devised that can be relied upon to render the coaptation of mesenteric margins safe and exempt from sloughing. The most reliable methods are unsafe and dangerous.

Experimentation has led me to the conclusion that while my anastomotic catgut and horseshoe plates are the best mechanical devices yet suggested, for the reasons given in my published papers, yet I am thoroughly convinced that no mechanical aids are at all necessary. By means of the plates the operation can be completed a little earlier, but the results are no better. The technique of intestinal anastomosis without mechanical devices is as follows :

All the preliminaries as to the preparation of patient, parietal incision, etc., I waive, and will only describe the work on the bowel. In case of a resection of ilium, the resected gut and mesentery are removed and the open ends of the bowel are each closed by means of the double running suture of silk. The intestinal contents are stripped back from each free end of the bowels for eight or ten inches, and the intestines handed to an assistant who holds the free ends in opposite directions, side by side, while the operator incises the two guts with a scalpel as far as possible from the mesenteric borders, making an opening double the size of the calibre of the intestine. The assistant does not release his hold during the entire procedure, but clamps the intestine sufficiently to prevent the return of its contents. After the openings have been made, the assistant slackens the parallel guts so the operator can

make the outside safety sutures on the mesenteric margin before the coaptation of openings is made.

I approximate the bowels, serosa to serosa, without denuding the serous surfaces;[1] the handling produces sufficient contusion and outflow of plastic lymph for all practical purposes. I use the intra-loop sutures of silk and then continue the outside safety sutures clear around the anastomotic opening. If the running sutures are used, leakage may result from a draw, tear or giving of the bowel. The free ends are anchored to the bowels by means of two or three interrupted sutures, and the mesentery closed by the same suture placed horizontal with the mesenteric bloodvessels. In large openings on the convexity of the bowel, where I have been in the habit heretofore of closing by means of my horseshoe catgut plates, I now close by means of intra-loop sutures (imitating the hinge) and making secure by means of the outside safety sutures, which gives the same result in increasing the gut calibre at the point of operation. In all operations for intestinal repair after resection, anastomosis should be resorted to. Whenever the bowels are brought together end to end, there is usually a disturbance of the blood supply at the mesenteric juncture to such an extent that sloughing in the majority of cases will occur.

In the operation of circulo-lateral enterorrhaphy[2] the bowels are approximated at the expense of the convexity of the bowel in such a manner as to increase the calibre of the gut to any extent desired, but it is an unsafe procedure. The operation recently devised by Dr. Briggs, of St. Louis, in which he turns back all the coats of the distal end of the bowel, except the mucous, like a cuff, and approximates by draw suture, is not free from danger of leakage from suture fistulæ, due to the fact that the sutures pierce all the coats of the bowel. While the omental graft may, in a great measure, lessen the risk of leakage, the danger is too great to recom-

[1] I have found denuding, as I have heretofore recommended, unnecessary.

[2] See experiments of Dr. W. E. B. Davis in the Alabama Medical and Surgical Age, August, 1892.

mend the operation as at all practicable. In fact, no operation that contemplates the approximation of the mesenteric border of the bowel is safe and reliable. The operation of circulo-lateral enterorrhaphy I formerly recommended from the fact that the intra-loop sutures are taken parallel to the vessel of the mesentery, and the blood-supply is not apparently cut off, but the risk is very great from cutting off the blood-supply, owing to the strain on the vessels. The danger is also frequently great, due to the cutting out of the safety sutures at this point of the approximation. Dr. Briggs has introduced the method of keeping the bowel outside of the abdomen until repair takes place. *If a circular enterorrhaphy is done, by all means the bowels should be kept on the outside,* not for the reason he recommends though, but because we never know when we are going to get a leak at the mesenteric approximation. Dr. Briggs[1] brings the bowel outside, because peristaltic action of the bowel, in a great measure, is controlled and the muscular jerking relieved. The tissues at the mesenteric border of the bowel are so frail and delicate that it is almost impossible to make an outside safety suture sufficiently tight without cutting or tearing the tissues, and I cannot indorse any operation that requires keeping of the bowel on the outside of the abdomen until reparation takes place. If, then, this is necessary we had better make an artificial anus, and close afterward by a secondary operation. The only operation for closing the bowels, end-to-end approximation, that theoretically recommends itself sufficiently to lead the operator to attempt it is that of circulo-lateral enterorrhaphy,[2] in which the bowels are approximated at the expense of the convexity of the divided ends, which prevents constriction and enables the operator to make the calibre any size desired; but more experiments with this operation by my brother have demonstrated that it is unsafe.

In many experimental operations in which the intestines were brought end to end all methods of suture would cause a

[1] St. Louis Medical and Surgical Journal for October, 1892, pp. 215-218.

[2] The Alabama Medical and Surgical Age, July 1, 1892, pp. 292-294.

constriction of the bowel at the point of union to such an extent that the calibre become incompetent for physiological purposes. Wherever the bowels are brought together end to end there is usually a cutting off of the blood supply to such an extent that sloughing, in the majority of cases, occurs; but in the operation herewith described constricting of the calibre is an impossibility, but sloughing is rare. There has been a constant desire on the part of the profession, since anastomosis has been perfected into a justifiable procedure, to do away with the operation; but, so far, all efforts have proven useless. By anastomosis the bowel can so easily be restored to its integrity, and with such little danger, that any other procedure, as a substitute, is seldom attempted.

Anastomosis is a procedure that is not only justifiable but cannot yet be easily done away with.

There are certain conditions of the bowel in which resection is not to be thought of, and to render the bowel competent a collateral circulation must be established; and to do this there is no means that is so justifiable, available, and easy as anastomosis. When we have a stenotic condition of the intestines it is often preferable to flex the bowel and approximate laterally rather than resect and restore the bowel by end-to-end approximation. There are stenotic conditions of the stomach and bowels that result from malignant growth and tubercular deposits, necessitating lateral approximation without resection. We can approximate the intestines end to end in such a way as to make the calibre any size we choose, but, as stated, the operation is unsafe, and I will now describe it only to condemn it.

After the resection the mesentery of the resected gut is left *in situ* and the ends of the intestines are brought together by intra-looped sutures that are tied on the inside of the bowel. This suture is first made on one side and then on the other of the mesentery. They are continued in interrupted loops until the bowel on each side is approximated to about one-half; then, to increase the calibre of the gut, an incision is made in the convexity of the approximated ends to any extent desirable or necessary (with corners trimmed off) to make the gut

calibre large enough for all physiological purposes. After this incision is made the line of loop sutures is continued until the two slits in the bowel are approximated just as the ends of the bowel. The last suture is tied and then pushed inside of the gut by means of a needle or small pair of forceps. After this interrupted outside sutures are rapidly carried around the line of union, approximating the serous surfaces beyond the needle openings made by the internal loop sutures. The bowel is lapped in the mesentery, which has been trimmed from the resected bowel, and held by two or three interrupted sutures. The mesentery becomes engrafted upon the bowel, greatly protecting the line of sutures, and preventing intussusception or invagination.

I have been able to succeed in protecting the bowel by the mesentery left from the resected intestine, but when we have been most careful and done the operation most satisfactorily leakage at the mesenteric junction has occurred and produced peritonitis and death. I am thoroughly convinced that any operation that gives so great a mortality as end-to-end approximation should never be resorted to. I have here only alluded to the operation, because I have heretofore in my enthusiasm recommended it, and now desire to place my condemnation on it as an unsafe procedure. The responsibility of recommending surgical procedures is very great, and an indorsement should only be given to such operations as have been thoroughly tested by experimentation.

In gastro-enterostomy the end of the bowel may be brought in direct contact with the stomach by these intra-loop sutures in such a way that the gastro-intestinal opening can be made any size desirable. In gastro-enterostomy, if the intestine is very small we only have to slit up the intestine and bring the lateral sides in union with the stomach.

The approximation in an entero-colostomy is made just as a gastro-enterostomy. In all operations of circulo-lateral coaptation the mesentery, if possible, should be utilized for protecting the line of sutures. Whenever this is done the mesentery is so shortened as to prevent the invagination and volvulus of the bowel at this point.

A CASE OF CYST OF THE BROAD LIGAMENT.

By D. Malloy Prince, M.D.,
Laurinburgh, N. C.

In these days, when any cross-road physician will venture to open the abdomen, it seems an act of supererogation to report a laparotomy. But inasmuch as there are some points connected with it not in common with ordinary cases, I venture to submit the following:

About one year ago Tildie B., a "gingercake-colored" negro woman, applied to me for advice concerning a swelling of the abdomen, which, she said, had been growing in size for nearly six months. At that time her health was excellent and all of her functions in good condition. She was of the opinion that she was "in the family way" (from her standpoint, a "consummation devoutly to be prayed for"), and came to me because she feared that it was "too good to be true," from the fact that she continued to menstruate regularly, both as to time and quantity. Physical examination showed a rounded tumor occupying the abdominal cavity, and in appearance and to the touch differing in no appreciable way from the pregnant uterus. There was some faint evidence of fluid, but, to the touch, the tumor was as firm as is the uterus.

The growth occupied the centre of the abdomen, and in position and outline was identical with the pregnant uterus. A vaginal examination revealed the cervix jammed up behind the pubic bone and almost infantine in proportions. The woman's breasts, too, were poorly developed; she had had no children, though married several years. The vaginal walls were very much relaxed and were thrown into rugæ.

The mucous membrane of the vulva presented the characteristic bluish tinge of pregnancy, although I felt satisfied that the woman was not pregnant. I told her to wait until after the usual period of gestation had passed, and then to report to me again; or, in case the tumor grew rapidly or her general health failed, to come back sooner. She went away disappointed and dissatisfied, because she had hoped and expected that the growth would have been pronounced to be a baby. In a short time she consulted my friend Dr. A. W. Hamer; he told her about as I had done, and still she was not satisfied, but sought another physician, Dr. K. A. Blue, with the same result.

Last May she presented herself again at my office, and I told her then that an operation was her only chance for relief. After consulting with her husband and friends, she decided to have the operation done, and on June 1, 1892, assisted by Drs. N. M. McLean, of Gibson Station; W. T. Pate, of Laurel Hill; N. L. Stule, of Rockingham, and K. A. Blue, of Laurinburgh; and in the presence of Drs. R. D. Dickens and A. W. Hamer, of Laurinburgh, N. C., and Dr. Westmoreland, of Baltimore, Md., I removed a cyst from the right broad ligament, weighing between twenty-five and thirty pounds.

An incision extending from above the pubes, a little way, to two inches above the umbilicus, was made in the median line, and the growth exposed. It was found then that the tumor could not be lifted out of the cavity, and we opened the sac and let the fluid escape. Not having in our possession a trocar of sufficient size, we resorted to a new method; this was to seize the edge of the rent in the sac with a pair of hæmostatic forceps, introduce a rubber tube into the sac cavity, draw the remaining loose edges of the rent tightly around the tube and secure it with another forceps. In this way we managed to empty the contents of the sac into a tub without getting more than a few ounces into the abdominal cavity. However, this would have made but little difference as the fluid was bland and innocuous. After the sac contents had been disposed of we found that there was no pedicle to the growth, and that it would be necessary to shell it out of its bed in order to remove it. This was done without much trouble and with but little detriment to the integrity of the sac walls. In two or three places the capsule was torn into shreds, but the

major part was left as a bag. The tumor was in the fold of the right broad ligament, and had twisted the uterus almost completely around, and had lifted it up out of its proper place. The Fallopian tube was stretched over the growth; the right ovary was intact; the left was degenerated and presented a cyst about the size of an English walnut. This was ligated with catgut and cut off with scissors. The sac came away entire. There was no opening into it save the one through which we had emptied its contents. There had been no bleeding except the oozing from the bed of the tumor. The question then arose as to the disposal of the capsule from which the tumor had been removed. Should it be cut away, or should it be stitched back and allowed to remain? We decided on the latter course.

In order to cut it away we would have to leave a broad stump, which would almost certainly adhere to the adjacent viscera, and there would be a large surface for infection. By stitching the capsule back into its bed we would have nothing but peritoneum facing peritoneum. So we proceeded to sew together the rent edges and to quilt down the up-torn surfaces of the capsule. This was done effectually, and we had no stump to deal with, nor was there any necessity for a drainage-tube. After flushing the cavity of the abdomen with boiled water, the edges of the incision in its walls were approximated with fifteen or twenty silk sutures—care being taken to include the muscular tissue, as insisted on by Dr. H. A. Kelly, and the operation was done.

The operation lasted about one hour, the patient being under the influence of the anæsthetic for nearly two hours. We finished the operation at 2 P.M. About 5 P.M. the patient's temperature was 99.4° F., and her pulse 124. The next day I did not see her, being away in attendance on an obstetrical case; but Dr. A. W. Hamer saw her for me, and reported her temperature at 102° and her pulse 142 in the evening.

The next day I saw her in the afternoon, and after that every day until she was well. Her temperature record and pulse-rate were as follows:

	Morning.		Evening.	
	Temp.	Pulse.	Temp.	Pulse.
June 1	...	...	99.4	124
" 2	...	...	102	142
" 3	102.6	142	103.8	150
" 4	100.8	120	102	130
" 5	101	120	101.8	125
" 6	101.8	128	102	118
" 7	101.8	116	102.8	125
" 8	101.6	116	102.4	125
" 9	101.4	113	102.4	123
" 10	101.4	113	102	115
" 11	100	101	101	117
" 12	99.8	108	...	...
" 13	99.6	108	...	...
" 14	99	102	...	...
" 15	98.6	90	...	...
" 16	99	91	100	102
" 17	...	...	99.2	95
" 18	...	...	99	91
" 19	...	...	99	91
" 20	...	...	99	84
" 21	98.6	97	...	...

After this her convalescence was uninterrupted. For the first three days her bowels were constipated, and on the third day she had a dose of salts; after this she was regular. Her kidneys acted all right throughout her confinement. Her appetite was poor all the time, and it was a hard task to feed her, as she could not endure milk. Eggs and whiskey, occasionally, and light bread and tea, with milk-shakes and ice-cream, constituted her food. Her tongue was pale and flabby until the ninth day, when it began to improve in appearance, and on the thirteenth day was clean. On the fourteenth day she sat up in bed; on the fifteenth day she sat up in a chair; and on the nineteenth day she was walking about the house. She stayed for a week longer, going about the town wherever she cared to go, and then went to her home, four miles in the country.

She was in my office on the 24th of September, nearly four months after the operation, perfectly well. There is no evidence of any enlargement at the former site of the tumor; the neck of the womb is in its proper place, and the uterus is freely movable. Her functions are all properly performed, except that she has never menstruated since just after the operation; then her

courses were profuse and lasted four or five days. She weighs now 145 pounds; with the tumor she weighed 156 pounds. Her general health is excellent. Sexual desire is unimpaired, and the act painless. She says she has not had an uncomfortable feeling since she went home, and that she is able to do as much work as formerly.

I could not account satisfactorily for the rise of temperature in the case, occurring, as it did, on the day of operation, so I wrote to Dr. H. A. Kelly, of the Johns Hopkins Hospital, asking him to give me his views on the subject. In reply he said:

"I feel sure that in the case concerning which you write me, you did wisely not to drain, as you might then have had some more serious infectious trouble. I think, undoubtedly, the symptoms arose from a mild localized septic infection, possibly the staphylococcus albus, which may have infected a blood-clot within the layers of the ligament. Where you shell out a tumor in this way it is exceedingly difficult to prevent some slight after-bleeding. In all such cases a careful vaginal examination will generally show some unusual fulness to one or the other side of the cervix. In such cases you do not always have any marked chill when the septic elements are pretty well shut off from the circulation. The source of infection would be either the finger-nails of the operator, which are prone to carry it even after the most thorough scrubbing, where the nails have to be used for taking out a tumor as in broad-ligament cysts, or such an infection may proceed, as it does in a large number of cases, from the uterine surface through the stump of the tube. I very rarely use the drainage-tube now, as it entails a variety of serious disturbances and is the actual avenue of the infection of sepsis in very many cases. Dispensing with it necessitates greater pains in checking hemorrhage and greater cleanliness before closing up the abdomen. Your case is a rare one and I would like very much to receive some fuller account."

In conclusion, I would say that the only feature of interest attached to this case, outside of its rarity, is the treatment of the bag-like cavity from which the tumor was removed, and the doing away with the drainage-tube. The after-treatment was simple in the extreme. She took no medicine except one

dose of salts and two ten-grain doses of quinine sulphate. This was all given during the first three days of her confinement. Notwithstanding her bad surroundings, a dirty negro hut, and her indifferent appetite, she made an excellent recovery, and is now, I think, entirely well.

THE CURETTE IN OBSTETRIC AND GYNECOLOGICAL PRACTICE.

By John G. Cecil, M.D.,
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The curette, like many other surgical instruments, has experienced seasons of favor and of disfavor. At no time, however, as at the present, has its usefulness been as fully recognized. So much have the dangers attending the use of the instrument been magnified, that unquestionably there yet lingers in the minds of many, strong prejudices against it. Many, in fact most of the objections, that justly obtained before the application of aseptic precautions are now no longer tenable. It can never be maintained that the use of the curette is entirely devoid of danger, but hedged about with all the modern methods of preventing septic infection, it may be stoutly claimed that that danger is reduced to a minimum in any case of whatever kind where its use is called for. So, in any case, where the indications are plain, the most cautious and timid need scarcely hesitate in its application. It is hardly necessary to add that the use of this instrument in the hands of a bungler, or careless manipulator, is capable of doing incalculable harm, despite aseptic precautions, let them be never so thoroughly instituted. The good results that may be had are so manifold and great, and so far outweigh the dangers, that to enter a plea for a place for it in the armamentarium of the obstetrician or gynecologist would be a work of supererogation. When I say curette I do not confine my meaning to those bent wire affairs which have all along been regarded as harmless, and we might safely add, almost useless, at least in gynecological practice, but to the modern improved instru-

ments with cutting edges. The indications for the use of a dull or a sharp curette depend upon the exigencies of each case, and must be left largely to the judgment of the operator. There is no doubt that much of the disfavor the curette has fallen into is due to the use of a dull instrument where a sharp one was demanded.

Before referring to the special indications for the use of the curette, and this part of my subject alone would lengthen this paper beyond its intended limits, a brief consideration of the preparations and methods necessary to its proper and safe use will not be inappropriate.

To begin with, curettage should be dignified by the name of an operation, and the antecedent preparations should be just as jealously carried out as those, for instance, for an abdominal section. Many untoward results have followed because the operator deemed it only necessary to place the patient in position, introduce an instrument, itself probably not clean, and scrape away some foregn body or adventitious growth. Concerning the technique of curettage, I cannot do better than follow the suggestions of that most zealous advocate of the procedure, S. Pozzi, in his recent superb work on gynecology. The patient should have a full bath the evening or the morning of the operation. The rectum emptied by enema, the bladder by catheter. The external genitals thoroughly washed with soap and water, and afterward with a strong antiseptic solution; vaginal injections of sublimate solution 1 to 2000 should be enjoined twice daily for several days prior to the operation. On the day of the operation three injections are to be given, the first two at intervals of an hour, the third at the very moment of the operation. It must be borne in mind, if bichloride of mercury is used for the douchings, that gynecological patients will safely stand stronger solutions than obstetric cases. Should the cavity of the uterus need powerful disinfection (as in certain cases of gangrenous fibromata, intra-uterine cancer, with putrid fungosities, etc.), it is advisable to extend the douching into the cavity. There may exist a demand for continuous irrigation; if so it is readily applied by

means of the irrigating curettes or other devices. Though pain in many cases is not great, yet for the sake of thoroughness and control of the patient, an anæsthetic is demanded. The operation may be satisfactorily done in either the dorsal or lateral decubitus. The vaginal walls are separated and held apart by retractors or a suitable speculum in the hands of assistants. The first step of the operation is to fix the uterus with tenacula or Museux forceps. The cervical canal must be sufficiently dilated to permit the easy passage of the curette. This is safely and quickly accomplished by the graduated or the steel dilators. The choice of a curette is, as has already been hinted at, not a matter of indifference. In general terms, it may be said that dull instruments are most suitable and safest for obstetric cases, while the sharp or cutting instruments are most serviceable in gynecological cases. The scraping of the cavity of the womb should be done in a systematic manner. Beginning at a certain point, say the posterior wall, every part of the surface should be carefully gone over until the starting point has been reached. During the progress of the scraping, if it is necessary from time to time to remove the detritus, this can be done by the spoon-shaped instruments or by the irrigating tube. If it is desired, the field of operation can be entirely submerged throughout the operation by an antiseptic solution, by simply elevating the hips of the patient and filling the vagina with the fluid. When satisfied that every part of the cavity has been curetted, it should be thoroughly irrigated with a hot antiseptic solution ; this followed by application of some mild caustic or packed with a strip of iodoform gauze. The patient should be kept in bed for three or four days at least, even in the simplest cases. Though, to emphasize the safety of this operation under antiseptic management, I have seen surprisingly good results follow the use of the curette in gynecological cases, with no bad effects whatever, that were operated upon in the University out-door clinic, and were allowed to go some distance to their homes, and where it is certain injunctions to remain in bed were not followed. It has been extremely gratifying both to Professor Anderson, with

whom I have been associated, and myself, that not one unfortunate complication has followed this method of treatment in the many cases that have been subjected to it, even with the rather incomplete antiseptic possibilities of an out-door clinic. But, even with so good a record to substantiate the foregoing, similar risks are not advised, only mentioned to demonstrate the possibilities of this treatment.

The special indications are so numerous that even brief mention of them in this connection will not be permissible. In obstetric practice there are two conditions that demand a curette, and demonstrate its usefulness most plainly; they are persistent hemorrhage, due to retained secundines or fungous degenerations of the endometrium after labor or abortions; and the septic conditions of the puerperal patient. It is in this class of cases that the dulled curette finds its greatest field of usefulness and yields the most brilliant and satisfactory results. Particularly in those annoying hemorrhages that follow incomplete abortions does the curette answer the demand. Instead of temporizing with ergot, hot douches or other hæmostatics, the curette is the certain, safe, and rapid substitute. Much time and annoyance is saved, and the subsequent progress of the case is most gratifying. In the management of septic conditions that follow labor, either premature or at full term, I am disposed to claim for the curette an important place. Any one who has made even a macroscopical post-mortem observation of the endometrium of a case of puerperal septicemia can see at a glance the indication for the curette. Here is a cavity the lining membrane of which is a decomposing mass. The most important part of the management of such a case is to cleanse this cavity and afterward to keep it clean. I do not advocate inconsiderate invasion of the puerperal womb; on the contrary, unless the indications are plain, am much opposed to it. I am satisfied that many cases, especially in private practice, are lost, because we are either too slow or else lack the courage to apply the remedial measures that give us the best and often only hope. I refer especially to the intrauterine douches and the curette. The douche should always precede

the curette, and it should have a fair trial, even to the extent of continuous irrigation. If the continuous irrigation fails, recourse to the curette is the *dernier ressort.* The curetting will not reach poison that has already been taken up by the lymphatics or bloodvessels, but it will limit the further production of it. With the curette the sloughing surface is bodily removed, and with it the focus of infection; in fact, we are treating this as we do any sloughing surface in surgical cases. It will be seen at a glance that this procedure should not be deferred too long, if we would reap the benefit. Many cases apparently hopeless may be saved by this active and radical measure. The limits proposed for this paper will forbid extended account of the application of the curette to many of the particular cases of gynecological character, or to detailed description other than what has previously been mentioned of the method of using it.

In those obstinate cases of so-called "uterine or cervical catarrh" that become such an annoyance to the busy practitioner and such a godsend to the poor, but aspiring young gynecologist, because they are not inclined to get well, of all the curative procedures that are lauded by their respective authors, none promise so well as a vigorous attack with a sharp curette. My own experience with the curette in this class of cases has been uniformly satisfactory. The curetting must be deep enough to remove the diseased follicles to their entire depth, and when this is done, certain relief will seldom fail. Painful and intractable cases of membranous dysmenorrhœa can be most effectively treated and cured with judicious use of the sharp curette. The operation should be performed just prior to menstruation. The small-sized submucous fibroid tumors that cause excessive and dangerous hemorrhage, can be brought under the benign influence of the curette with signal advantage. The hemorrhage in these cases is not so much from the tumor as from the uterine mucosa, which is kept in a constant state of congestion and irritation by the presence of the fibroid. Much can be said of the value of the curette in the management of cases of uterine cancer that are beyond the

reach of the more radical operations. The unfortunate patient can be made more comfortable to herself, her friends, and attendants; the rapid progress of the destructive process may be in a measure abated, septic infection can, for a time at least, be warded off, and alarming hemorrhages can sometimes be anticipated and put under more perfect control. The scrapings of the curette can be utilized for diagnostic purposes when cancers are suspected in the body or fundus of the womb.

Enough has been said to fully demonstrate the usefulness of this instrument in the gynecological field. While much may be said of the indications for the curette, much has also been said as to the contra-indications, perhaps too much. Many modern authorities seem disposed to ignore such contra-indications as have become classic, namely, acute inflammation in and about the uterus and its appendages, and, also, chronic inflammations in the same regions that have left the womb fixed by many adhesions, and which seem disposed to rekindle upon slight provocation.

I cannot yet bring myself to the point of advocating the bold use of this instrument in the presence of such conditions, when the results may be so dire and regrettable.

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